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A METHOD OF MAPPING RETINAL CIRCULATION BY PROJECTION.[1]

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The object of this investigation was to study the entoptic phenomena of blood movement in the retina, and to obtain maps by an introspective method which should indicate the course of the vessels in as satisfactory a manner as possible. Numerous superficial observations have been made along this line, but reference to physiological and psychological literature failed to bring to light any detailed maps of the blood movement as observed subjectively upon a bright field. Maps in varying degrees of satisfaction have frequently been traced by many of the 'shadow methods,' *i. e.*, by actual projection of the vessels upon a screen. These, however, reproduce only the larger vessels with any degree of definiteness, and do not indicate the direction of the blood flowing through them. This method is here employed only as a means of verifying the other work.

§ 1. HISTORICAL.

The perception of the phenomena of blood movement in the eye seems to have first been described by R. W. Darwin in 1786. Darwin states that he could see these movements when gazing at the sky or some other bright field, after holding his breath and rubbing his eyes. The experiment was thus facilitated by an increase in the amount of blood sent to the eye. He believed the appearances to be dependent on the general state of the observer's health, and to be most distinct after the eye became fatigued.[2]

[1] From the Psychological Laboratory of Cornell University.
[2] C. J. Burch: The Retinal Circulation, *Nature*, LIII, 558.

In 1819 J. E. Purkinje published his observations,[1] and noted that the retinal circulation might be observed subjectively under certain conditions. By directing the attention on a clear flash of diffused light, he could see bright spots appearing in continuous courses which emerged at irregular intervals at the same points, completed their courses, and disappeared. Purkinje also laid down a method for projecting the vessels as shadows. This he accomplished in a dark room, by directing a strong light on the sclera. When the light was moved slightly from side to side, the vascular shadows became visible in the glare before the eye.

J. Mueller called attention to these blood movements in his Handbuch der Physiologie des Menschen, published in 1840 (II, 390). He indicated that an expression of such movements might be observed by glancing at a bright even-toned field, such as the sky, a patch of snow, or a white paper uniformly illuminated by a steady light. He saw nothing save confused movements, which rushed towards one another and passed one another in irregular smoke-like courses. Mueller considered it impossible to determine the direction of the movement. He accepted it as a manifestation of the retinal circulation. In addition, he noted certain definite phenomena occurring when one stoops and rises again suddenly, which he attributed to congestion caused by the ebb and flow of the blood. These movements were described as a "leaping" of dark bodies in various directions over the field.

O. N. Rood, in an article published in 1860,[2] states that by gazing intently at the bright sky or other illuminated field through three plates of cobalt glass he was able to obtain a view of "small bodies like animalcules" which travelled over the field in all directions. The spots seemed to be yellowish in color, and appeared elongated in the direction of their motion. A convex lens held before the eye did not alter the occurrences, a fact which indicated that these movements were situated in the retina. Near the axis of vision, the moving body seemed always to pursue the same direction, and to disappear at the same point. Rood concluded that these phenomena were probably due to blood corpuscles.

These bodies may also be observed through a certain thickness of a solution of cupro-sulphate of ammonia, though not as perfectly as with the cobalt glass. Through red, orange, yellow, green or purple media they are still less distinct. Rood advanced a theory, in explanation of these appearances, as follows. The blood discs are yellow, and therefore opaque to blue and violet light. They, consequently, cast shadows when such media are interposed. But since the retina is strongly impressed with blue light, the portions protected by the corpuscles experience a complementary sensation, and instead of a dark shadow, a yellowish moving speck results. Since yellow media are transparent to red, orange, yellow and green lights, no very perceptible shadows are cast when these are employed.

In another article,[3] published a few months later, some additional observations were noted, mainly the facts that faint indications of movements may be seen with the naked eye, and, further, that the moving bodies are sprinkled over the field, and do not appear in compact masses. Quoted in Rood's paper are some of the results of W. B. Rogers. Following Darwin and Mueller, in their observation that

[1] Beobachtungen u. Versuche zur Physiologie d. Sinne, I, Prag, 127.
[2] The American Journal of Science, XXX, 2nd series, 264.
[3] *Ibid.*, 385.

the phenomenon is facilitated by an increase in stimulation of the retinal blood supply, Roger maintains that after continued effort of vision, active exercise, or other stimulation of the circulation, these dots are readily seen in broken curves, which come and go in such a manner as to indicate the presence of certain prescribed and permanent channels. As one continues to fixate a bright field, a shade covers one's vision, and one sees, upon a dark background, innumerable streams of particles which move in loops and curves and retain a constant pattern. These courses have a "tawny yellow tint," and last only one or two seconds. The shade passes, and returns after an interval, though the courses are then less distinct.

Rogers indicates several methods for obtaining this projection:

(1) gazing through a black paper tube at a white surface till the eye is fatigued and greatly excited;

(2) gazing for a few seconds into a pocket microscope held at about focal distance from the eye;

(3) a method previously indicated by Purkinje: the light of a spirit-lamp, with a salted wick which gives an intense yellow flame, is concentrated on the eye by a convex lens of three-inch focus. The bright field thus observed soon resolved itself into a mass of small, round, densely-packed moving bodies which appeared light against a darker ground. They seemed "packed together like a fine mosaic," but their paths could be traced as they moved at a slow uniform rate through the narrow channels. Rogers adds that if yellow plates of glass were interposed the bodies appeared more distinctly. He attributes the indistinct vision, which is concomitant with eye-weariness and faintness, to an increased congestion in the vessels.

Helmholtz in his Optics[1] reviews the subject, and states that Vierordt saw a rushing movement when he agitated his spread fingers before his eyes. This movement was attributed by the observer to retinal circulation. Helmholtz affirms that neither Meissner nor he himself has been able to see such movements; they find only an appearance of "shoreless streams." However, he does not question Vierordt's phenomenon or its explanation. The observations of Purkinje and Mueller are also cited, and, in the light of these, with the directions furnished by Rood, Helmholtz concludes that the phenomenon undoubtedly proceeds from blood movements in the retina. In explanation, he advances the theory that the corpuscles become jammed together in large masses in the smaller vessels, so that the portion of the capillary tube ahead of the check is emptied. At last this blockade is released, and the whole mass moves quickly on. This causes the appearence of definite movement through certain restricted portions of the smaller vessels. Similar phenomena are observed in capillary circulation under the microscope. Helmholtz concludes that the bright spots observed should be considered as the optical expression of "little stoppages" in the retinal blood flow, which occur only in certain narrow passages of roots of vessels.

Landois and Stirling[2] cite Boisser as advancing the theory that the appearances may be caused by the red blood corpuscles in the capillaries. These, acting as small light-collecting discs, concentrate the rays falling on them from without, and throw the light upon the rods of the retina. In order to accomplish this, each corpuscle must be in a special position,—probably with its broad face toward the light. If it rotates, the phenomenon disappears. This theory is interesting, but (as will be shown later) the facts of observation and histology do not substantiate it.

[1] Handbuch der physiologischen Optik, 2nd ed., 1896, 198.

[2] Manual of Human Physiology, 1895, II, 995.

§ 2. Experimental.

Method. After a careful consideration of the methods indicated in the literature, a simple proceeding was adopted as follows. A rectangular screen, 67x87 cm., of even-surfaced, white, translucent paper was erected in a window facing the western sky, the light from which was unobstructed by trees or other objects. This screen was marked off in squares of 2 cm., and at the center was placed a fixation-point. Before the screen a desk was arranged with drawing-board, on which were placed papers with cross rulings like those on the screen, which brought the eye in a line with, and 92 cm. distant from, the fixation point. An adjustable chair for the observer, blue glasses for media, and shades for the unused eye completed the apparatus. Each observer worked with the right eye, the left being comfortably shaded so as not to interfere with the vision of the other. Observations were made both through the blue glass and with naked vision. The moving points were observed, and their tracks carefully noted by aid of the arrangement of squares, and then transferred to the map.

In attempting to check or verify the results obtained in this way, all the classical methods of shadow-projection were investigated. To enumerate a few:

(1) In a dark room a candle is held near the eye which regards the distance. The vascular courses are supposed to appear as dark lines on a yellowish background. These courses come out distinctly when the candle is moved.[1] This method was tried, but no very satisfactory results were obtained.

(2) The Purkinje method. A strong light is focussed on the sclera at a distance as far as possible from the cornea. When the light is moved slightly from side to side, the courses appear before the eye. This method requires delicate handling, owing to the burn and the general strain caused by the focussing of a strong light upon the eye. No very satisfactory phenomena were observed, and the method was not found to be practical as a means of verification.

(3) W. C. Ayres[2] has enumerated several methods involving modifications of those already described. The most important requires a dark room, and an even, constant source of light. This light is reflected through the pupil on to the retina by means of such dull reflectors as a gold ring or a teaspoon bowl. The shadows are thus projected as in the other methods. Ayres can see them very distinctly on a white card held a short distance from the eye, and has been able to trace a map of the macular region which is surprisingly intricate in detail and regular in contour.

A close examination of Ayres' map, however, discloses the fact that only 12 courses were traced with any definiteness. The fine net-work of anastomoses between these is arbitrarily indicated. He is able to note the fovea roughly, but states that he cannot differentiate veins from arteries, though at times he perceives some indication of the direction

[1] B. A. Randall, in de Schweinitz, Am. Text Book of Diseases of the Eye, Ear, Nose and Throat, 1889, 140.

[2] In *Archiv. f. Augenheilkunde*, Wiesbaden, 1884, XIII, 29.

of blood-flow. With this method, as with the method from which it is derived, it is necessary to keep the light on the retina in motion, by a side movement either of the reflector or of the head. This, of course, introduces an error in judgment when one attempts to map the vessels. We attempted to reproduce Ayres' results, with a trained observer, but our success was slight, owing to the great difficulty experienced in directing the proper light upon the retina.

(4) The second Purkinje method has already been outlined as it was worked out by Rogers. We repeated the experiment, and noted the movements described; but they were not nearly so numerous as those obtained by gazing at the bright sky, nor as satisfactory, owing to the very limited field which the lens allows. We also noticed that muscæ volitantes appeared frequently and proved distracting, as it was hard to differentiate them from the vascular courses.

(5) The method adopted is the simplest and for many reasons the best of those which the literature indicates.[1] It consists in glancing through a moving pinhole or steropaic opening toward an evenly illuminated screen. Both openings were employed, but the steropaic has the advantage, since by agitating it in various directions considerable portions of the retinal field are exposed. Vertical movements when the opening is held horizontally bring out the transverse courses; horizontal movements with a vertical opening show the longitudinal vessels. They appear fully as definitely as Ayres' courses. The fovea is not normally projected, nor is there any indication of the direction of the movement; but both these points are satisfactorily given by our working method. It is, accordingly, with this verificatory method that we are concerned, the rest having been discarded as more complex and in general less satisfactory.

It may be of interest to note that an investigation was made into the possibility of obtaining a photograph of the retinal vessels. The literature upon this subject was worked over, and a number of methods studied, but it was found that the experiment is as yet too primitive to furnish any satisfactory record of the smaller vessels or capillaries. Since these are the courses of main importance in the present work, the idea of photographing the retina was abandoned.[2]

§ 3. Introspective Results.

Maps were obtained from four observers in the manner detailed. The general characteristics of their observations were uniform and not greatly at variance with the results obtained

[1] de Schweinitz, 140.

[2] C. Panel: D'un moyen pratique de photographier le fond de l'oeil. Paris thesis, 1887.

W. Thorner: Ueber d. Photographie d. Augenhintergrundes. Berlin thesis, 1896.

G. Aarland: Internationale medizinische-photographische Monatsschrift. Leipzig. II, 1895, 4.

Ludwig Jankau, in same volume, 18.

A. E. Fick: Bericht ü. d. zwanzigste Versamm. d. ophthalmologischen Gesell. Heidelberg, 1899. XXVII, 197.

Th. Guilloz, in Revue medicale de l'est. Nancy, 1895. XXVII, 212.

by the authorities above quoted. Two of our observers gained their most satisfactory data after interposing a blue glass between the eye and the field of vision. The other two worked for the most part with the naked eye. It was noticed that the dots were less plentiful but slightly more distinct when seen through the blue glass. This may be accounted for by the fact that on the blue field there seems to be a darker ring about the spots, which aids to differentiate them from the background. Retinal rivalry caused by the black screen over the left eye was experienced to some extent by all the observers, but it was quickly overcome, and did not return after the first few sittings.

The more detailed notes made by the observers, whom we shall designate as *B* (Mr. T. Bliss), *W* (Dr. M. F. Washburn), *C* (Miss J. A. Cochran) and *O* (Mr. R. M. Ogden), are as follows.—*B* procured the most elaborate map of all. He is a careful, patient observer, and seemed to obtain, without trouble, definite courses over all parts of the field of vision. The more remote courses were usually longer and perhaps less fine in detail; still they repeated themselves frequently, and seemed to offer little difficulty as to their exact position.

Throughout the field *B* differentiated the appearances into two classes. (1) As to motion: there seemed to be a class of spots (*a*) which moved very rapidly through a considerable distance, and merely indicated their general direction. These were, for the most part, situated beyond the macular region. Their courses appeared as mere curves or straight lines. There was also a class (*b*) of slower moving spots, whose courses were more definite. They traversed shorter distances than the others, and were situated for the most part about the macula. (2) As to color: *B* used the blue glass exclusively, and observed spots both lighter and darker than the background. The lighter spots were the largest and most numerous. In general, they appeared to have dark borders and to be of a faint bluish tinge. The darker spots were smaller, less distinct, and infrequent. *B* has also seen spots which appeared at first as dark shadows. As they approached the center of the field they brightened for a brief interval, then darkened, and finally disappeared as they came.

The phenomena which are supposed to result from fatigue, as described by Rogers, were all carefully noted by *B*. His results demonstrate that such appearances could be seen, but not at all frequently, nor in a very satisfactory manner. By gazing through a black tube at a bright field he noted one course like a white line. This may have been a particle in the humor, since such bodies not infrequently appear as white, thread-like lines. Courses of the same type, together with a general movement over the field like a gentle ripple of water, were seen when glancing through a lens held against the sky. Two courses thus picked out were checked on the other map. Similar projections were obtained by squinting the eye and glancing toward the screen without the mediation of tube or lens. These may also have been caused by muscæ volitantes, or by shadows cast by the eye lids or eye lashes.

W worked exclusively with the naked eye. Through all her experimentation she has been unable to procure very satisfactory courses in the right portion of the visual field. She obtains a number of well-defined paths, which constantly repeat themselves, in the region roughly indicated as from 8 to 18 cm. to the left of the fixation point upon the projection screen. These are so far removed from the center

that it is impossible to check them by shadowing the vessels themselves. Several expedients, such as cutting off the light from the left portion of the field, were resorted to, in the hope of forcing the attention on the right half; but they proved of no avail except, perhaps, to bring out the macular capillaries somewhat more clearly. By stimulating the left eye, in which *W* could see movements on the right or nasal side, and then changing to the right organ, we were able to bring out a few slight movements in the smaller vessels to the right of the fixation point. At last, by the use of a stationary pinhole through which the screen was fixated, a few satisfactory tracks about the center were procured. This restriction in the phenomena of circulation in *W*'s eye can be attributed only to a persistent habit of attention which seemingly cannot be overcome. The courses to the left of the field run through considerable distances, but are not noted with any of the fine detail which the vessels themselves are known to possess. Yet the general direction of the flow is definitely mapped, and, by reason of numerous repetitions of the same movement, we must accept the tracks as fairly accurate.

The nature of the spots themselves was usually lost in the observation of their movement. At times, however, elliptical or circular spots were described, which were generally lighter than the surrounding field, and possessed an irregular, dark border. The paths near the macula were more definite and perhaps more rapid than the others.

C's results were greatly handicapped by a fatigue which always set in early in the test, and put an end to reliable tracing. This fatigue caused the appearances to be blotted out entirely, and was accompanied by a distinct loss in power of concentration. The increase in the stimulation of the retinal flow, which has been noted as attendant on fatigue of the organ, was thus discounted and rendered useless. *C*'s best results were obtained with blue media. In regard to the character of the dots, she made the same general differentiation as *B*, viz.: that small, slow-moving dots operate near the macula, while larger, clearer specks move in a rapid, jerky manner through the remoter parts of the field. The tracks mapped about the fixation-point were frequently repeated, and in many cases checked with the actual vascular projection. Those further out in the field were for the most part indefinite.

O gained his most definite results in the macular region. There were times when movements in the periphery were noted, but they were not reproducible except as to general direction. Fatigue usually occurred after from 15 to 20 minutes' observation, and was accompanied by diminution of attention which rendered further experiment difficult. Many more courses were seen without the blue glass than with it, but they were not so distinct. *O*'s best results were obtained by a successive use of the two methods. In general, the spots were brighter than the background, though occasionally vague, moving shadows were perceived. Movements were commonly more rapid in the periphery. Courses varied in length from mere flashes to clearly defined tracings. The spots were devoid of color, save when the blue glass tinged them with its hue. On a bright day the spots sometimes had dark centers, and darker borders surrounding a light ring. The shape was not definitely noted, but seemed to be regular in contour.

O reported the following interesting phenomenon. "One day, when I was removing a dark screen before the window, I glanced at the field, and saw it filled with round, intensely bright spots, distinctly larger than usual. They darted about at a very rapid rate. This only continued for a moment, and, as the eyes became adapted to the light, the spots dissolved into the type of spots normally observed." It

seems possible that this occurrence was due to the suddenly increased stimulation of the retina when the pupil was much dilated.

§ 4. Conclusion.

This study has demonstrated that with a fair amount of patience and attention one may secure a satisfactory map of the movements of the blood through a considerable area about the region of the *macula lutea*. Patience and attention are essential, in view of the numerous objective sources of error. It is very difficult to maintain a fixation-point. When the eye slips away, the courses are materially displaced upon the projection plane. This is particularly noticeable with the method for projecting the shadows of the vessels, in which the observer views the field by intermittent light.

Any increase in the stimulation of the blood supply should produce a corresponding increase in these manifestations, as has been noted by many writers. But for definite and careful observation such increased stimulation is usually of no avail, since under the conditions the attention is at a very low ebb.

The physical basis for these movements is undoubtedly to be found in some peculiarity in the behavior of the blood corpuscles. The Rood theory, before mentioned, is based upon the supposed yellow coloration of the spots. *Not one of our observers could detect any yellowness.* This fact seems sufficient to render the theory untenable.

The Boisser theory has certain attractive features. Its assumption of the action of single corpuscles in focussing light upon the retinal rods will account satisfactorily for those isolated, sudden flashes of brightness which most of our observers reported. On the other hand, certain histological facts speak against this theory. The red blood corpuscles are bi-concave, and differ very slightly in density from the lymph which surrounds them. They are, therefore, calculated to disperse rather than to collect rays of light, if they have any refractile power at all.

The Helmholtz theory treats the phenomenon as the "visual expression of little stoppages" in the capillaries, which cause an emptying in that small portion of the vessel which immediately precedes the obstruction. These brief interstices conduct the light to the rods, and, by contrast with the surrounding field, produce the phenomena of bright dots. Histology demonstrates that brief checks in the blood-flow occur at times and may be caused in either of the following ways: (1) the corpuscles jam into, and pile up in, certain narrow vessels; (2) corpuscles often catch for an instant at the point where a vessel branches, and so produce a momentary stoppage. Helmholtz attributes the observed movements to the first of these

causes, and notes that the phenomena occur only in "certain narrow passages of roots of vessels." Our method, however, has clearly demonstrated that the majority of movements observed take place in vessels of an appreciable size, since they can be readily 'shadowed.' It furthermore seems difficult to believe that an empty space could precede a mass of corpuscles through such long distances as are frequently noted. The mass, when released, should spurt on, quickly filling the near intervening spaces.

It seems more plausible to connect these 'stoppages' with certain brief, jerky flashes which are noted through the field of vision, and to attribute the more prominent phenomena to chance spaces between corpuscles or bundles of corpuscles in the normal flux. The latter appearances may be in some manner aided by a varying refractile power of different portions of the vascular walls. We cannot note this as a fact, but the consensus of opinion among our observers favors the idea that the dots continually repeat themselves through the same portion of a vessel.

The fact of movement is noted before the shape or form of the moving body is perceived. But a careful introspection demonstrates conclusively that the definite, more numerous bodies always appear brighter than the surrounding field, and are regular in contour. We are thus limited to Boisser and Helmholtz for our explanation; but, as has been shown, the two theories admit of considerable criticism. After eliminating all errors, the Helmholtzian notion is perhaps most easily modified into a form which will comprehend our results. The spots are bright; they must, therefore, be conditioned by the passage before the retinal elements of media which readily transmit light. We know that spaces occur between bundles of corpuscles. When the external light pierces such an interstice, the rods behind it will be subjectod to an increased stimulation throughout the distance through which such a space retains its character. This little flash of brightness is then projected in a more or less circular form, and, by contrast with the surrounding field, appears as a light moving dot.

The darker rings and centers, following shadows, and gray appearances in general, may all be attributed to the brief, vague shadows of the corpuscles themselves, when they are collected in considerable numbers. They usually precede or follow the bright inter-spaces.

The rate of movement appears to be determined for the most part by the character of the vessel. Brief stoppages in the flow may offer some slight differentiation from the normal motions. Nearly all our observers note short, jerky movements which might be attributed to such checks in the flow. Yet, as a rule,

the rate of movement is so irregular that we cannot state with exactness when the phenomena should be attributed to a check in the flow, and when to a mere separation of bodies in their course.

Peripheral motion has been judged by one of our observers as faster, by another as slower, than central. The macular vessels are smaller than those in the periphery; therefore the flow would move most slowly in the macular region. But it is difficult to note the details of a movement which takes place in indirect vision. For this reason the courses in the borders of the screen are reported merely as movements in a certain direction, whereas the nature of the paths which are projected near the fixation-point can be reported with great detail.

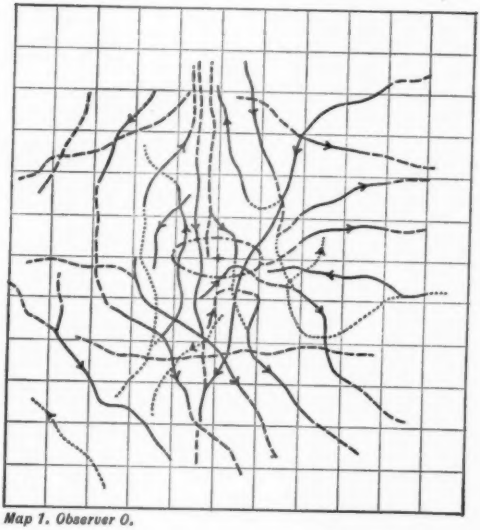
The Maps.

Continuous black lines represent courses obtained by the appearance of moving bodies upon the screen, and checked by the projection of shadow courses in the same position.

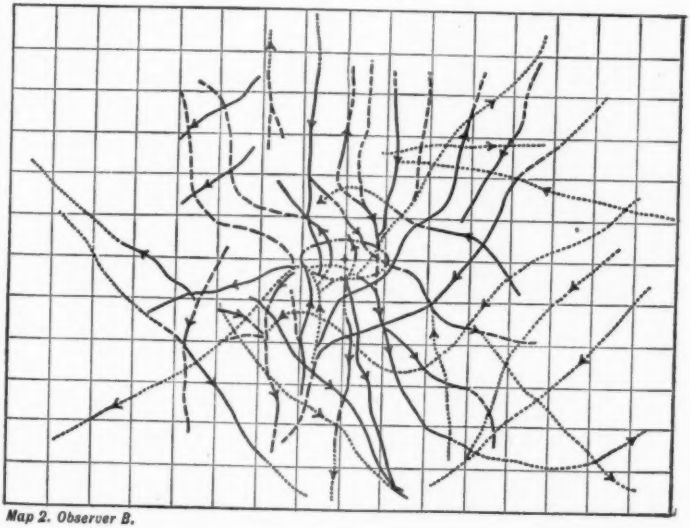
Dotted lines represent courses obtained in the same manner and many times repeated, but not verified by the shadow projections. This indicates in general that they are very fine vessels which have been overlooked or are too small to appear projected as shadows.

Broken lines represent definite courses which appeared with use of the moving pin-hole or slit opening, but through which no movements have been observed.

The elliptical figure about the fixation-point represents the macula lutea.



Map 1. Observer O.



Map 2. Observer B.

ON THE PSYCHOLOGY AND PHYSIOLOGY OF READING. II.[1]

By EDMUND B. HUEY.

In reading, then, at the ordinary distance,[2] say twelve to sixteen inches, the eye gets its data by a process of photographing successive sections of each line, the photographs overlapping constantly, and being taken at quite irregular distances. The conditions determining the points to be fixated have not been worked out. Introspection gives some suggestions about it which would be easy to write here, but which I believe to be entirely untrustworthy and perhaps misleading.

In reading lines of this length, from three to six fixations are made, usually four or five. But one line is read at a time.[3] In my experiments, about 80% of the line was traversed by the eye, the indentation being greater at the right. There were few retraceals, averaging about one in seven lines.

The forward movements seemed to occupy a little over 40σ, somewhat irrespective of the arc traversed, within certain limits. The return sweep was usually without interruption, and occupied from 50σ to 60σ.

The pauses occupied a variable time, averaging somewhere about 190σ. In fast reading the speed seemed to be gained by lessening the number and duration of the pauses.

It may be interesting to note the average number of words read per fixation. I append results of some representative readings of passages containing from nine to thirty lines each. The data were obtained by experiments with the direct attachment apparatus.

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[1] Continuation of paper published in this *Journal*, July, 1900.

[2] It seems surprising that no study should have been made of the optimum reading distance. Writers here and there advise this or that distance, but the advice seems to rest upon tradition, or upon observation of normal practice, rather than upon any experimental study.

[3] This has been so in every case, I think, which I have tested. The subjects were usually directed to "read for the thought by their own method." Of course, there is a "skipping and skimming" process with which we should all be familiar, in which both lines and thoughts are omitted.

ARTICLE.	LINE-LENGTH.	DISTANCE.	WORDS PER FIXATION.
Cosmopolitan Magazine,	121 mm.	33 cm.	1.83
"	121 "	33 "	1.50
American Journal (10 pt.),	98 "	35.5 "	1.96
" (8 pt.),	98 "	35.5 "	1.91
" (10 pt.),	98 "	33.0 "	1.70
Cosmopolitan Magazine,	60.5 "	33 "	3.63
"	43 "	33 "	2.60
"	37 "	33 "	2.44
"	30 "	35.5 "	1.94
"	25 "	33 "	1.58
"	25 "	33 "	2.16
"	21 "	33 "	2.17
"	21 "	35.5 "	3.33

It will be seen that with the short lines more words are read per fixation, though there is much variation, and little regularity of correlation shown between line length and words per fixation. Such variability is to be expected with the very short line lengths, when we consider how unusual they are, and how apt the eye would be to revert to old habits of moving a certain number of times each second, or for each phrase, or couple of words. In the shorter line-length, many of the movements were evidently from habit and not from necessity; as, *e. g.*, the eye would remain fixated while three or four lines were read, then shift a little and repeat, then zigzag, irregularly perhaps, For the reading of one of the 21 mm. passages above, I entered the following note: "In only three cases is a back and forward movement noticeable. Thirteen appreciably different fixations occurred. One fixation lasted two and one-half seconds, while the reading of whole passage (21 lines) occupied but about six seconds." With lines 25 mm. long, several lines would be read without shifting the eye. Toward the last of a 30 mm. passage the side to side movement was almost nil.

In all cases, with whatever line length, the eye moves oftener than would be necessary to get the printed matter within the range of clear vision. Thus, in my exposure experiments to determine extent of reading field, I read correctly, at first exposure of 15σ duration, one-third of such lines as these of this *Journal* article, on an average of several hundred trials; and this without help of context. Very nearly the half of such lines was read (seen clearly, as guessing was not allowed) from time to time, often enough to make me certain that my eye was capable of dealing with that extent of printed matter, when conditions of printing, arrangement of subject-matter, etc., were favorable. But with this possibility of three and a third to five words of average length per fixation, I actually read, as shown in the tests, about 1.9[1] words per fixation, photo-

[1] The average would be a little more than two words per fixation, if retraceals, breaks in return movement, etc., were excluded, as would

graphing the line at four or five points, even six sometimes. The reason for this can be discussed best when we know just what points are fixated and what effects various arrangements of printed matter may have upon the size of fixation sections.

Rate of Reading.

The reading rate varies greatly with the individual and with the subject matter. G. J. Romanes[1] found readers who read four times as fast as others of apparently equal intelligence and culture. Dr. Quantz[2] studied the matter extensively. He found as great individual differences as are given by Romanes, and states that the fast readers retain more of what they read than the slow ones.

I reget very much that Dr. Quantz has not carefully stated his method of determining the rate of reading. The rate is peculiarly apt to be affected by the conditions under which the test is made. Subjects mean such different things by "reading" a passage. I believe they usually have a tolerably constant rate for a given class of matter, about which they fluctuate according to circumstances. But it is difficult for either reader or experimenter to know whether this standard is being used in any given test. Again, it has not been shown that the fast readers for one class of matter may not be the slow readers for another. The possible speed always, and the actual speed in my own case at least, varies very much with the reader's apperception for the subject matter read.

The studies made thus far on the rate of reading, so far as I have been able to discover, give us no assurance that these sources of error have been eliminated; and knowing the difficulty of making the tests, especially with untrained subjects who so readily misconceive directions given them, I am convinced that we yet need (1) a thorough study of individual differences in rate based upon a *number* of tests taken at different times upon the same kind of reading matter, and repeated for diverse kinds of reading-matter; taking into account the subject's apperceptive relation to each class of matter; (2) a full and careful statement of the methods used in determining rate, if the results are to have anything of final scientific value.

I arranged a series of experiments, having for one of its objects to throw some light upon the matter of rate in reading,

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be fairer for this comparison. Besides, the sections are irregular in size, and often three or four words, at least, are actually read at one fixation.

[1] "Mental Evolution in Animals," p. 136.

[2] Psychological Review, 1898.

and the possibility of increasing it; mainly hoping, however, to get insight into the subject's methods of reading, through the opportunities for direct observation and questioning which the experiments would afford. I do not offer the results as final determinations of rate for these subjects, though for the (uniform) class of matter used, I believe, they are of comparable validity with the results of rate-tests usually made thus far.

Eleven pages, each containing 405 words, were selected from an interesting novel which presented no peculiar difficulties to the reader; the pages having a somewhat similar arrangement of paragraphs, and being of as nearly equal interest and difficulty as possible.

These eleven printed pages were read by each subject, a page at one reading, the time being taken with a stop-watch. The reading was done at one sitting, in a quiet room, free from distraction, with the subject comfortable.

The first page was read silently, at normal speed and by the subject's "usual method," "the way you like to read"—the only other instruction being, substantially, that he read "continuously and for the thought." The second page was read exactly as the first. For the third page the subject was directed to think the words over in terms of sound, to auditize it, but at normal speed, and for the thought. The fourth page was motorized, at normal speed, but without lip-movement. The fifth was as the fourth, but *with* lip-movement. The sixth page was read aloud at the subject's most natural rate. The seventh, eighth, ninth, tenth and eleventh pages were duplications of the preceding as to method (except that but one page was read by "silent normal" method), but were read at maximum speed. In the readings aloud, the subjects were required to read loudly enough to be understood at a distance of fifteen feet at least.

The subjects were given preliminary practice for each page, and were not allowed to read until they understood clearly what was required. In all the readings, and especially in the "silent normal" readings, the greatest care was taken to have the subject fall into the mood in which he would do such readings in an easy-chair at home; and I believe that the results approximate somewhat nearly, at least, what would be obtained under such circumstances.

Below are the results for the twenty university students tested:

The table gives opportunity for various comparisons which may be of suggestive value to those who may work further upon these problems. It will be noticed that the individual differences found for "Silent, own method" reading are found to hold in lesser measure for readings by other methods, for

readings at maximum rate, and in the averages at the right from readings by all methods.

Subject.		Silent Own Method	Silent Own Method	Auditory	Motor. Lips Closed	Motor. Lip Movement	Aloud	Average
R	Normal Rate	8.8	10.8	10.5	10.9	9.8	4.4	9.20
	Max. Rate	13.5	——	10.8	12.0	8.7	6.4	10.28
B	Normal Rate	7.3	8.4	6.7	6.6	6.7	4.2	6.65
	Max. Rate	8.8	——	7.7	7.3	7.4	5.1	7.26
A	Normal Rate	7.0	7.7	7.0	5.4	4.1	3.8	5.83
	Max. Rate	10.7	——	10.6	9.5	6.7	5.5	8.60
M	Normal Rate	6.7	6.3	4.5	5.5	5.4	3.7	5.35
	Max. Rate	9.3	——	5.9	7.5	5.9	4.9	6.70
Q	Normal Rate	6.7	6.1	4.9	5.5	5.7	3.5	5.40
	Max. Rate	9.2	——	6.1	6.1	6.0	3.9	6.26
N	Normal Rate	6.6	7.3	7.9	7.9	6.6	4.7	6.83
	Max. Rate	8.8	——	9.6	10.6	8.7	5.5	8.64
E	Normal Rate	6.6	6.7	6.0	5.1	5.3	4.6	5.72
	Max. Rate	8.1	——	5.3	6.4	5.6	4.9	6.06
L	Normal Rate	6.1	6.9	5.1	6.7	5.3	3.5	5.60
	Max. Rate	12.4	——	7.7	7.3	4.8	5.1	7.46
C	Normal Rate	6.0	6.3	5.4	5.1	5.0	3.3	5.18
	Max. Rate	8.0	——	8.2	7.7	6.3	4.9	7.02
G	Normal Rate	5.5	5.6	4.9	4.4	4.6	3.7	4.78
	Max. Rate	7.0	——	5.6	7.2	6.0	4.7	6.10
S	Normal Rate	5.3	5.1	4.6	4.5	4.4	3.1	4.50
	Max. Rate	12 4	——	6.0	6.1	5.5	3.3	5.55
O	Normal Rate	5.0	6.3	5.0	5.0	4.2	3.8	4.88
	Max. Rate	7.1	——	6.3	6.4	6.0	5.5	6.26
H	Normal Rate	4.5	5.7	5.5	5.7	5.1	3.7	5.03
	Max. Rate	7.6	——	6.2	7.1	5.8	5.4	6.36
D	Normal Rate	4.1	4.7	4.1	4.2	4.3	3.0	4.07
	Max. Rate	6.5	——	5.3	6.3	5.4	4.2	5.54
J	Normal Rate	4.0	4.6	3.9	5.0	4.3	3.5	4.22
	Max. Rate	6.2	——	5.2	5.0	5.1	4.6	5.22
F	Normal Rate	3.9	4.7	3.8	4.6	4.4	3.2	4.10
	Max. Rate	5.9	——	5.4	5.6	4.7	3.2	4.96
T	Normal Rate	4.0	3.9	3.7	4.0	3.7	3.6	3.82
	Max. Rate	5.9	——	5.5	6.0	5.2	4.3	5.38
P	Normal Rate	3.5	5.4	4.4	4.5	3.5	3.4	4.12
	Max. Rate	9.7	——	5.6	5.6	4.8	3.9	5.92
K	Normal Rate	3.1	3.3	2.3	2.7	2.7	2.2	2.72
	Max. Rate	3.6	——	3.1	3.7	3.2	3.4	3.40
I	Normal Rate	2.4	2.5	2.1	2.5	2.6	2.2	2.38
	Max. Rate	3.5	——	3.0	3.6	3.3	2.9	3.26
Av.	Normal Rate.	5.35	5.91	5.12	5.29	4.88	3.55	
	M. V.	25.7%	23.1%	25.8%	22.5%	22.4%	13.1%	
	Max. Rate.	8.21	——	6.45	6.85	5.75	4.58	
	M. V.	25.5%	——	24.6%	21.2%	17.3%	16.5%	

NOTE.—The reader having slowest normal rate above had a segmental affection of the retina, which doubtless affected his speed.

The next slowest was a Japanese student who, however, had studied in American schools for a number of years. The numbers represent words read per second.

Throughout the experiments I was most interested in getting insight into the reader's method of reading. Lip-movement was usual with but two or three. One of these was one of my fastest readers, though for those unaccustomed to the method, the lip-movement evidently hindered speed.

By far the largest number seemed to be of the audito-motor type, emphasizing in various degrees the auditory or the motor elements. Readers often indicated their usual method clearly by the ease with which they comprehended and used it when assigned, and by again and again reading pages by the assigned method in almost the exact times used for the "own method" pages.

A strong rhythmic tendency was observed, and this aspect of reading merits a careful study. Readers fall into a natural rate, which gives almost exactly the same times for page after page. (I from time to time tested readers upon several additional pages, by the various methods, and especially by their accustomed method.) Habits of eye movement would seem to be an important factor in setting this pace. For example, the second page readings in the above table were from a page containing fewer lines than the first, though the lines were of equal length, and the only difference apparent was in the inter-spaces between words. The average times for reading the two pages were almost exactly proportional to the number of lines in each, for a somewhat larger number of subjects than are given in above table. I find by experimenting upon lines marked here and there by crosses for fixation without reading, that the eye readily falls into a very uniform rate of progress corresponding more or less closely to its usual rate in reading.

I was constantly impressed with the fact that reading may go on in motor images without any apparent traces of movement of lips or tongue. The movement seemed "up in the head" to many of the subjects.

The fact that we constantly hear our own utterances, has, acting with other factors, indissolubly welded together the auditory and motor elements. I am satisfied that these elements are never quite dissociated in normal reading; and that what subjects call auditizing, or motorizing, is a combination of the two, usually in more nearly equal proportions than their early introspective accounts would indicate.

A purely visual reader is certainly not an impossibility, theoretically at least. The direct linking of visual form to ideas, cutting out of circuit the somewhat cumbrous and doubtless fatiguing audito-motorizing mechanism, would seem to be a consummation to be wished for, from some points of view. When the proper preliminary investigation of the reading-process has been made, this will be one of the most important sub-

jects of pedagogical consideration. Practically, however, I have not found the purely visual type.

Perception of Reading-units.

My various experiments in the exposure of reading matter had for one of their main objects to give suggestions for a rational point of view from which to regard the whole matter of perception of reading-units. I am satisfied that such direct and continued contact with the processes as they go on under conditions which can be controlled is the best road to right theory in this difficult field.

I offer a tentative view of the matter, which may be generalized all too soon as I well know; but which, I am convinced, shows leadings toward the truth that will appear upon fuller investigation and more mature reflection.[1] Much is to be gained by regarding the perceiving of letters, words, and phrases as phenomena of association. When a letter or word is seen, the most habitual associate tends to appear in consciousness, in preference to less habitual ones; and the habitual associate will come so quickly as to fuse with the first if the association has been inveterate enough. Every letter, combination of letters, syllable, combination of syllables, word, combination of words, phrase, etc., has associates more or less habitual. Not only does the perception of the letter or word arouse the idea of its absent associated letter, word, etc., but, when the printed associate follows, its perception is facilitated in proportion to the extent of the habituation.

The child learns to read, either by associating the visual forms of letters with their names or with their sounds, or (in the word method) by associating the visual forms of word-

[1] My study of reading was interrupted in the spring of 1899, while yet in the observation stage, as I had first planned that it should extend over another year. I made a temporary summing up of the study at that time, but refrained from publishing in the hope that my prospective teaching duties would not prevent my making a more satisfactory conclusion of the study, or, at least, would permit my making a better and fuller presentation from the considerable amount of data already on hand.

I have not since found time or strength for further experimentation or reflection upon the subject; and regretfully publish it mainly as it stood in the earlier form.

Among the later and valuable literature which has appeared, the article by Bryan and Harter on "The Telegraphic Language" (Psych. Review, July, 1899), has been of especial interest to me. Starting from a quite different point of departure, the authors, it seems to me, reach much the same general conclusions as to the perception of reading-units as those to which I have been led in my study of the reading process. It has, of course, been encouraging to find this at least partial corroboration of my theory in the work of other and experienced investigators.

wholes with word-sounds and meanings. The association between the optical form of the letter and its *name* is not strengthened by later reading, and disappears, comparatively, in favor of the association with the letter's *sound*. This is clearly shown in my earlier experiments,[1] and in the still earlier ones of Prof. Cattell, in which the naming of isolated letters required more time than the speaking of short words.

Whether or not the association of the letter's visual appearance with its sound is arbitrarily memorized by the child in learning to read, it comes just as truly and certainly as he practices reading. A pupil taught to read by the word method first associates the optical form of the word as a whole with the sound of the word without linking parts of this sound with particular parts of the optical form, *i. e.*, with letters; and so his reading may go on for awhile. But gradually, even if he has never been taught that the optical form is composed of letter units, he will note the likeness of the crooked beginning of "star" with the crooked beginning of "slipper," *e. g.*, and will form an association of this crookedness with the hissing sound noticed as occurring in both words. The association of the optical form of the letter with its sound thus arises and soon becomes inveterate. Doubtless the appearance of letters at the beginning and end of words facilitates the linking of particular sounds with particular letter-forms; but it would come in any case; and I think it tolerably certain that, whatever the learning-method, the reader must and does come to feel the force, visual and auditory, of individual letters before he reads with much facility.

Now this optical crookedness and this hissing sound are comparatively seldom found alone, and occurring as they do with other optical shapes and other sounds tend to call up these other shapes and sounds when presented, and call up preferably and most quickly those with which they have been most often associated, other things being equal. If the visual "a" has most often had "x" appearing at its right, the sight of "a" will, other things being equal, tend to arouse the visual representation of "x," and the *sound* of "a" aroused by association with its optical form will tend to arouse the sound of "x" preferably. Of course the optical form and the sound "a" have been associated with many other letters, and the associative tracts representing these will also be aroused more or less. The associative tracts representing "z" and "q," letters seldom given with "a," will scarcely be aroused at all. Now if the word "ax" is suddenly exposed or appears in reading, the sounds corresponding to "a" and

[1] *American Journal of Psychology*, July, 1898.

"x" will at once come up as the most inveterate associates with these optical forms. But the optical form "a" will call up also its preferred associate "x;" and the sound of "a" will do the same for the sound of "x;" with a strengthening of the optical and auditory "a" by "x" also in proportion as "a" has more or less often preceded it as compared with a probably preferred "e" (in suffixes, etc.). Perhaps the association of optical forms from right to left may be as facile as that from left to right if we accept the apparent fact that the eye receives no data during its movement forward.

In a longer word such as slipper, "s" may subarouse the forms and sounds of various other letters than "l," though the association to "l" is facile as compared, for example, with that to "x." "l" has more or less frequently been associated with "i" following, and tends somewhat of itself to call it up as compared for instance with calling up "x." But the combination "sl" has far more frequently been given with "i" than has "l" when "l" has stood alone, and thus the effect of "s" preceding "l" is much to facilitate arousal of tracts representing "i." The combination "sli" tends to arouse comparatively few letters, such as "p." "t," "c," "m," "d," etc., and the trend of association is more and more constrained as less of the word remains.[1] The combination "nigh," for example, would almost invariably arouse "t," its almost invariable associate. In general, it only requires a direct application of fundamental principles of association to justify the statement, confirmed, however, by its agreement with the facts of observation, that letters have more or less preferred associates according to their habitual arrangements into words in a given language; and that letters presented in these preferred sequences mutually strengthen the visual and auditory perceptions of each other, and thus arouse the apperceptive complex representing the visual form of the word and its sound. When letters in nonsense arrangement are exposed, subjects often state that they have clearly seen many more than they can repeat to the experimenter. The letters as optical forms tended to call up their preferred letter-associates, but these rather hindered than helped the perception of the adjoining letters, and there could be no apperceptive knitting together into a complex which could be remembered.

The perceptions of the various parts of a letter shoot together into the perception of the whole letter, the part perceptions mutually assisting each other according as, from being often presented together, they have habits of interassociation; and knitting together into the complex perception-whole "a,"

[1] See James's Psychology, Vol. I, p. 365, *et al.*

"x," or what not. This first fusion seems to occur below the threshold of what we ordinarily term consciousness. The visual perception of the letter-unit is instantly supplemented by more or less of audito-motorization of its sound.[1] The perception of a word occurs similarly except that here auditory and motor (tactual), as well as visual elements enter into the fusion, in various proportions as the reader tends to the visual, auditory or motor type. The letter perception-units shoot together again in the perception of the whole word, the letter-perceptions mutually assisting or hindering each other according as the printed arrangement follows or violates habits of interassociation. There is the direct fusion of the *visual* letter perception-units; and the indirect but similar fusion of the auditory and motor elements that are linked with these visual units. This fusion into a word-unit is probably below the threshold of consciousness, for the most part, in most reading.

Word-perception is facilitated or hindered, it is true, by other factors than past co-presentation in vision, hearing, or speech. Certain auditory or motor elements blend easily with certain others, and the perception of these harmonious combinations will, of course, be comparatively facile though the combinations be new. The intrinsic difficulty of other combinations may overcome the tendency to facile perception incident to habitual interassociation. There are other factors which would have to be taken into account in an exhaustive treatment.

Now the knitting together into letter-wholes of data given from printed letter-parts seems to begin the instant the parts are presented, quite automatically, and may occur simultaneously at all points throughout as much as half the present *Journal* line-length, as it seemed in some of my experiments on extent of reading field. Subjects would state that they saw the letters clearly as letters; though they could not be remembered long enough for reproduction unless their transient life was reinforced by union into the more stable and permanent word-unit; and still better if this word-unit could be reinforced by union into some characteristic word-group; the higher complex units saving all their elements from falling into speedy oblivion.

Exactly similar are the readers' habits of association from word to word, and from phrase to phrase. The exposure of the word "A" beginning a sentence subarouses many of its past associates, preferably substantives or descriptive adjec-

[1] I allow myself here to use the term perception for processes which are ordinarily unconscious or subconscious. Perhaps, however, my meaning is as evident as I could make it with a more accurate use of terms.

tives. If "large" appears after it, the possibilities as to what may be the third word are narrowed to a manageable list, which are perhaps all subaroused. If "juicy" follows, the associations for the fourth word are still more limited, and often one or more of these is made so much more probable by the context or particular situation that the reader's apperception scarcely needs the appearance of the word "apple" to complete the phrase. On the other hand we can easily understand that the appearance on the page of a word violating this order of expectation would have its recognition hindered rather than helped by the existence of this apperceptive expectancy; and we are thus prepared to understand why the reading of nonsense matter takes about twice as much time as that of sense passages.

I shall defer further discussion of the perception of word-groups until I have given account of some experiments which I have made upon the associative and interpretative processes in reading.

Interpretative Processes in Reading.

Of the interpretative processes in reading there would seem to have been little experimental study thus far. The subject seems difficult to approach, yet of the greatest importance and interest. I arranged the following tentative experiments in the hope that, whatever the direct results, they might suggest a helpful method for further investigation.

Two passages of at least average interest and of only moderate difficulty were selected, one a description taken from a magazine article of how a spider spins its web, the other from the introduction to Percy's Reliques, describing the arrangement for the entertainment of Queen Elizabeth at Killingworth Castle. The passages contained some seventy-five words each. These were typewritten, and the lines cut out and pasted end to end on strips of cardboard, so as to make sense continuously throughout in a single long line.

From other typewritten copies the single words were cut and pasted on small pieces of cardboard. These cards were then shuffled, and were exposed, by means of a krypteon,[1] to the subject, one word at a time, in haphazard manner. Before the exposure, a ready signal was given, and the exposure lasted four seconds. The subject was seated comfortably in a quiet room, and was directed to look at the exposed word, and allow associations to play as they would in any direction.

The exposure of the sense matter was under similar general conditions. The first word of the passage was exposed, then

[1] Described on page 403, Sanford's Experimental Psychology.

the first two, then the first three, etc., a new word being given at each exposure along with the preceding context, the subject attending mainly to the new word in each case. In a few cases two or three words forming a phrase were exposed together in the sense passages, but usually but one new word was exposed.

Another sense passage of 130 words from a rather interesting magazine article on "Tribal Religions" was exposed, in phrases of two to five words, from beginning to end continuously, with context always in sight. To illustrate the character of the division into phrases, the following are some representative ones: "Political party," "Among their own countrymen," "Of the Pharisees," "Declared that," "It is true," etc.

Three subjects have been tested on the first series. The passage exposed by phrases has been given to one subject only. About a month was allowed to elapse between the exposure of the isolated words and of the context passages, which they formed.

The results show characteristic differences between the associations from isolated words and from words given in context for all subjects, though in most respects the subjects have very considerable individual differences.

The words given in isolation gave a much greater variety of association than did the context words, though the total amount of associated contents suggested by them is considerably less. When the isolated word appeared there was usually an indefinable recognition of the visual form of the word as familiar; and accompanying or very closely following this (probably the latter, though the subjects are not explicit), the word is usually "mentally pronounced." One subject whom I shall designate as "A" practically always had this mental pronunciation; another to be designated as "B" had it almost always, often with some associated word as "Atlantic cable," when "cable" was exposed, or "Can-can," when "can" was exposed. The third subject to be known as "C" quite frequently mentioned that the word was motorized, when first exposed, but more frequently did not mention this. This subject showed much more tendency to think in visual terms than did the others. The motorization of a word would seem to have often been present and disregarded, as was discovered sometimes by questioning. Usually, however, the subjects were not questioned at all, but dictated as fully and accurately as they could just what had been in consciousness during the four seconds, doing this immediately after the end of the exposure; the experimenter recording their account as nearly in their own words as practicable consistently with the necessary condensa-

tion. It may be mentioned that the subjects were all university men and accustomed to introspection. This subject "C" very frequently had associated phrases come up a little *after* the beginning of the exposure, and these were almost always mentally pronounced.

The subjects were generally unable to say after careful introspection whether this mental pronunciation meant motorizing, or auditizing, or a combination of the two. They inclined in general toward the latter view, emphasizing the motor or auditory elements according to circumstances.

After the visual recognition and mental pronunciation of the isolated word was apt to come a mental pronunciation of some phrase or other word in which it had often occurred to the subject: as "Sweet by-and-by" from "by," "Himself, herself, itself," from "himself," "Vertical writing" from "vertical." The word was rather especially apt to suggest some line of poetry which would often be but dimly suggested, leaving the subject with a vague and tantalizing feeling of something which he could not get. This occurred much more often with subject "B" than with the others. The vivid arousal of the *feelings* belonging to words and phrases which were thus but subconsciously aroused was a phenomenon often occurring with him. In general, words showed a tendency to call up groups with which they had been rhythmically connected. Words were often pronounced "interesting," "agreeable," "full of meaning," or the opposite of these, and occasionally these judgments seemed to refer to the sound or visual appearance of the words themselves; but more usually the feeling seemed to be traceable to some particular associations or uses of the word in past experience; and though this reference could not always be made, as was the case also with the suggested phrases from poetry, still it seems probable that the feelings were usually associatively mediated by words or situations which do not any longer appear above the threshold.

As has been said the associations from isolated words were of the most varied character. The word, "top," for instance, gave a visual picture of a hilltop, then the motorized phrase, "Top of hill," then another mental picture of a hill with disappearing base, then a mental picture of a flagstaff on the hilltop at home, then a visual picture of a top given to the subject as a present in his boyhood days, and memory of seeing it wound up and spun, with the memory of the singing noise which it made. Again the motorization of an exposed word would suggest another similar in sound, and the association would start from this. The word would sometimes be divided and associations taken from different parts; or the

word would be given different significations in different parts of the exposure time.

One of the most striking things brought out was the lack of association from connective and relational words, definitive adjectives, etc.; and the displeasure with which they came consequently to be regarded. They seldom aroused any ideas directly, and few associations of any kind except verbal ones, usually phrases of which they customarily form a part. Occasionally they gave evidence of setting the subject's thoughts in characteristic directions of expectancy; and doubtless the prepositions, especially, always had some very general influence in determining how the whole thought organism should face the coming related object. These vague expectancies were occasionally noticed by the subjects, particularly in the case of such words as "between," "into," etc. The whole feeling of the subjects toward these words and their inability to call up associations irresistibly suggested that the mind had no place for them as separate wholes, and that there was no normal way of thinking them except as more or less fused components of larger units; viz.: as parts of phrases, and perhaps sentences as they continually occur in reading.

Turning to the associations from words given in context, we find as their most distinguishing characteristic that they are far less variable. The mere statement that the word to be exposed is part of a reading passage limits the trend of association when no context has been given. The limitation extends further when the subject has caught the general topic discussed in the passage, and still further when the exposed word is given upon a verbal and ideational background formed by preceding context. In the case of the word "top," for example, after the mention of "web-weaving" the word "top" no longer suggests "top of hill," "flag-staff," "spinning tops," etc., but preferably the "top of a post or gateway," with "spider-situation" in mind, and a greater vividness of the suggested picture for its having already been partially aroused in expectation.

This difference in the trend of association is shown by all subjects, but much more by some than others; and it varies from time to time as the subject's greater or less interest made him more or less approximate the condition of normal reading.

The newly exposed word was usually mentally pronounced as before, and "fitted into the preceding," as was very often remarked by one subject; the new word contributing apparently toward a notion of sentence unity, to which each additional element added a needed part. Immediately following this there was in a majority of cases a filling out of the sentence or phrase so as to make sense with the preceding con-

text, and when this did not occur there was usually a "forward push," "forward tendency," "tendency to fill out," as it has been very frequently described by the subjects. All subjects have emphasized the strength and comparative constancy of this feeling, and mentioned it as perhaps the most striking thing to be observed in the experiment. It was not present in any considerable degree at the beginning of paragraphs, nor at the close of sentences and paragraphs. The "little words" (as the subjects came to call the words expressing relation, etc.), gave but little except this forward feeling and verbal associations. They seemed, as subject "A" remarked several times, to be but "verbal counters" in the sentence.

Subject "A" showed comparatively little tendency to visualize throughout the experiment. There was, however, visualizing of some of the main objects and scenes referred to in the passages read,—enough to form a vague background for the story, which seemed, however, in the main to be thought in verbal terms. Subjects "B" and "C," however, had more of the visual element, and the interpretative process with them seemed to be more or less independent and parallel with the verbal associative complex. In the story of the spider's weaving its web, for example, a visual picture of a spider was early formed, which was present throughout, though more or less modified to suit the different references to it as the story progressed. The spider was seen in a visual background that had different components fused into it in a kaleidoscopic fashion, as the story gave additional data; but no violent breaks were made. While this scene would pass out of the attention field sometimes as some substantive would call up scenes peculiar to itself, it constantly remained as a factor controlling the course of expectation and association.

This visualization was almost always static. The spider jumping was visualized as the spider ready to jump or just alighted. The thought of motion, when mentioned at all, seemed to be one of tendency to movement in the subject's motor organism.

The agreement or disagreement of the exposed word with the trend of expectation produced by the preceding context was a matter of frequent remark by the subjects, and often of considerable feeling on their part. No matter whether or not the subject had consciously formulated his expectations, there was a feeling of rightness or wrongness about the sequences, which was expressed in judgments of fulfilled or disappointed expectation. That this disappointment or fulfillment of expectation was not caused merely by getting or failing to get the particular verbal form desired, irrespective of its intrinsic fitness, is evidenced by the frequent judgments that the given word

though different was "all right," "still better," etc. Sometimes the new word given really closed the sentence to the subject's expectation, and the author's appending some corollary caused displeasure.

It would be profitable, if I had time and space sufficient, to give a detailed account of the results of exposure by phrases of the article on "Tribal Religions." A few comments must suffice, though I hope to make more use of the data, and especially of the method of experimenting, at some later time.

In almost every case the phrase was first "read and motorized," as the subject put it. It would seem as though this meant a recognition of the visual form first with a closely following motorization; though just what the "read" meant was rather uncertain to both of us. The processes usually seemed simultaneous to the subject's introspection. (I might say the subject was a thoroughly trained observer). Following this there was almost always either a "fitting in" of the word-group with what preceded, or a "filling out" with some word or group of words; according (usually) as the exposed phrase made closer connections with the preceding or following context. The fitting in with the preceding would be expressed in such words as "joined with what came before;" "recognized that this was sense expected though not the words;" "felt the fulfillment of expectation though words were quite different from expected;" "Sense fell in with the expected sense;" "gave phrase its place in sentence as far as could;" "gave it its place as completion of sentence;" (these are from subject's dictation, paraphrased sometimes).

The filling out would not always be with definite words. It was often expressed as "tendency to complete," "tendency forward," "very strong tendency to fill out." Quite usually, however, definite words or phrases came to mind, completing what was given. The subject remarked on one occasion that this forward tendency was by all means the most prominent of the things to be observed introspectively. There was occasional dissatisfaction at the non-fulfillment of expectation.

The subject had shown comparatively little tendency to visualize in my previous experiments with him, and showed no more in this. He remarked on one occasion that throughout the experiment he was struck with the little amount of visualization as compared with the verbal association. On another occasion he remarked that the words served simply as "counters" till he got "the whole thing."

There was a vague general picturing of location of what was described, and some vague visualization of main scenes and characters. But for the most part by far the subject was concerned with words and their interassociations. There were in-

frequent tendencies to translate into other terms, as when "condemned" seemed to suggest a gesture of striking down with the arm. These translations were usually but incipient and rather intangible.

I feel the need of much more experimenting, and especially of much more time than I have had for reflection upon the associative and interpretative processes in reading, before attempting any final account of them. Provisionally and roughly, I should say that in reading there were two sets of processes, somewhat independent and paralleling each other: (1) a reading in terms of interassociated word and phrase units (themselves composed of interassociated sub-units), thought in a variously proportioned combination of visual, auditory, and motor elements; (2) a reading (or interpretation) in terms of direct representations of the realities with which the subject matter deals; a picturing in sense terms of what the words symbolize.

The relative prominence of the two processess varies greatly with the individual, and, of course, with the subject matter as well. The first is the constant process, is the major part of the performance for most readers; is the part which makes the heavy draft on the psychophysical machinery—is the fatiguing and delaying process. It is the *ding an sich* for the average reader.

The second process is a sort of dramatization in which the reader sees and hears and smells and tastes, and takes a part. Consciousness may almost desert the first process in its interest in the scenery of the second; yet this scenery is constantly being changed by the word-workers behind; and it may be jarred to confusion by a wrong arrangement of word or phrase.

Usually, however, and with some readers always, the first process has much of the consciousness. Here, however, with practiced readers at least, consciousness has mainly to do with the higher phrase and sentence units. The reader's mind has a complex tangle of interassociated words. Each word and class of words has its preferred associates; and when these come in the habitual sequences their perception is duly facilitated. No matter that given phrase or word group has never been seen before. Its perception will be duly facilitated, and it will be unitarily perceived if it is cast in a habitual form—if it has, *e. g.*, familiar sequences of object following preposition, or of substantive following appropriate adjective. Much of the whole matter, I believe, may be worked out from this point of view.

I have been interested in noting the part which motorization seems to have in this higher knitting together of word-units into phrase or sentence-units. The word, with myself at least, seems to be motorized as soon as singly presented, instantly

when *seen;* and this motorization seems to help hold it in consciousness while it is combining with the other words into the higher unit, the phrase, which is then itself motorized (or in reading aloud is spoken) by one unitary effort.

It is well known that in reading aloud the vocal utterance follows several words behind the eye's fixation point. It seems to me, also, that in silent reading there is a similar phrase motorization (or auditization, or both as is most usual) following behind the eye, and *after* the perception and audito-motorizing of the single words.

(This, of course, has reference to readers who motorize; and it seems difficult to find readers who do not, in a greater or less degree.) The single-word motorization does not make so much noise in consciousness as the later and reinforced utterance as part of a phrase; but is truly there, to my introspection at least.

Of the second process I can give but slight account. Most words with which we deal in reading are concepts; not representing definite single realities, but having more or less vague abstract ideas which they symbolize, and which they may or may not call to consciousness when presented.

Most of these concept words, as relational words, verbs, etc., represent abstract ideas which are very intangible; of which little clear account can be given upon the most careful introspection. Doubtless in reading there is usually a vague consciousness of the generalized experience which these words represent;—but for the most part they seem to be in the reader's consciousness as mere words; translatable in some measure at the reader's will, but the reader seldom willing. The words representing particular realities or less generalized experience more often call up these their associates.

Much of the translating seems to come not from words singly perceived, but from the perception of phrases and sentences as wholes; the words acting as "counters" until blended thus.

I have been more and more interested in the verbal side of reading. It seemed at first as though reading was dead, inane,—really *not* reading at all unless there was constant translation into the realities symbolized; but I have found various good thinkers and workers in science who seemed to be predominatingly verbalists in their reading;[1] and I am not sure but that the most of us read by far the most of our words and phrases without appreciable translation.

Such verbal readers and thinkers may be analogous to a banker who does an immense business in terms of drafts, bank-

[1] We might expect this when we recall the results of Galton's tests of the imaging power of men of science.

notes, checks, etc.,—controls all sorts of situations by them, is free to convert them into property or whatever else they represent, at any time; but would be much hampered if he actually had to do this converting very often. So such a reader carries on his reading and thinking in a kind of short-hand, uses a mental algebra, lives in a word-world, a world of symbols. He can thus be more systematic, precise, expeditious; and after all his method may not be so fundamentally different than that of the reader who habitually translates into images; for the latter is but a dealer in other symbols of the same realities; symbols which he takes comfort in thinking are more like the realities than those in which the verbalist revels.

Such use of words, however, cannot and should not come until a broad and deep basis for it has been laid in terms of experience with the realities and with the images which more nearly represent them. Words, except as they are correctly and intelligently convertible, are certainly most deceptive and dangerous symbols for the reader as for the thinker.

Practical Suggestions.

In concluding here what must remain an unfinished study, I am tempted to add a word as to the possibilities of improvement in arrangement of reading matter and in reading method.

I was led to the present study in considerable part (1) by chafing at the slowness with which we must traverse pages of books and papers, the mind appearing able to assimilate the thought much faster than the eyes can traverse the lines, or the voice the words; (2) by curiosity to know the immediate conditions of the peculiar fatigue caused by reading.

I am far from being able to conclude as to cause or cure of either condition. I am firmly convinced, however, that there is possible an arrangement of printed reading-units which will greatly lessen the work of the eyes and considerably lessen that of the mind, and which will increase the speed.

The present arrangement compels the eye to cover three or four times as much ground as is necessary to get its data. It causes unnecessary difficulty in "keeping the place," and causes a certain amount of continual distraction from the presence, in the upper and lower periphery of vision, of comparatively unrelated matter. This, however, is but a beginning of the arraignment which might be made of the present arrangement from the eye standpoint.

From the interpretation side, one of the serious objections is that the present arrangement makes "skimming" difficult and unsatisfactory. We have noticed the progressive tendency to read in larger and larger units; and this should go on until much of our reading could be done by a skimming process.

This skimming should be but an enlargement of normal reading, proceeding by a somewhat regular series of omissions and resting places, in which, however, all the matter could be taken account of in some degree. At present, however, one who attempts to "skim" down a page must proceed in a kind of hurdle-race fashion, breaking across lines of which the full content is necessarily unknown; and violating at every instant reading habits which it has taken years to form. The arrangement that is finally found to be the best for ordinary reading, will, I believe, facilitate skimming as well.

Again, improvement is to be looked for in a more systematically and logically organized subject matter. The reader's habits of word and phrase association and expectation have not been consulted in composing in the past as they will be when the psychology of style has been made a matter of common knowledge. The fact that subject matter arranged to accord with the reader's reading-habits is read in one-half the time of matter arranged contrary to these habits, suggests the immense advantage that may come from studies in this field.

It may well be that greater speed will come through a better method of reading from pages even as now printed. I await with interest the appearance of some study of reading rate among persons who have been forced for any cause to use a purely visual method. I should expect that such reading could be done at a faster rate, though possibly with disadvantage in some unlooked for direction.

I have tried various devices for increasing my own speed in reading, and have succeeded with but one, viz., to get thoroughly alive to the subject and keep *trying* to read fast. This seems to cause associative work to be done more glibly; there is more "reading inside" with fewer clues needed from the outside, and so, probably, fewer and shorter eye-fixations. In any reading we construct the thought and the words anew from inside; and if we lazily wait to *do all* this constructing after each eye-full of data is given, there is much time lost, and much room given for extraneous and distracting mind-contents. Personally, I should be grateful if I had been given speed drills in reading for thought, in my public school days.

Any adequate treatment of the matter of fatigue in reading is out of the question until the analysis of the reading-process has gone much further; especially as there is much to indicate that the fatigue is a matter which concerns the brain as much as it concerns the eye; and that the cumbrous associative word machinery may have much to do with it for most readers.

Eye-fatigue will be considerably lessened if publishers of books and papers will more constantly observe certain minimum requirements as to size and thickness of type, spacing,

quality of paper, etc., upon which investigators are in practical agreement. As to the vertical separation of the lines (in printer's phrase "the leading") and line-length, there is some disagreement; but a tendency, on the whole, toward the shorter line-lengths and a reasonable amount of "leading." It seems preferable to some writers, however, as Javal, to enlarge the type at the expense of the "leading," if necessary.[1]

But the greatest lessening of fatigue, at least of eye-fatigue, may be expected when there has been an entire reconstruction of the forms in which reading units are presented to the eye. The fossils of form perpetuated by spelling[2] and printing traditions need to be ground in the hopper of common sense, and reformed in the light of science and of that same common sense; and it may be found, upon trial, that the idols which the spelling reformers have been seeking to overthrow are not more pernicious (or perhaps more tenacious of life) than these which the printing iconoclast is soon to attack. At any rate, it is high time that we put the question whether we are doing the best that we can in our arrangements for inter-communication of thought by printed symbols.

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[1] On the subject of printing-norms, valuable suggestions will be found in articles by Javal (*Rev. Scientifique*, 1879 and 1881), Cohn (*Rev. Scientifique*, 1881), Sack, reviewed by Erisman in *Zeitschrift für Schulgesundheitspflege*, Nos. 4 and 5, 1898), Griffing and Franz (*Psych. Review*, 1896), Blasius and Ludicke (*Vierteljahrschrift f. off. Gesundheitspflege*, Bd. XIII, p. 432), Sanford (*Amer. Journal of Psych.*, I), Cattell (articles in *Mind* and in *Wundt's Studien*).

[2] Note, *e. g.*, that through the retention of the useless silent letters, the eye and the mind must deal with about one-sixth more data than is needed. See "The Spelling Reform," by Prof. F. A. March, published by U. S. Bureau of Ed., 1893.

STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF THE UNIVERSITY OF MICHIGAN.

Contributed by W. B. PILLSBURY.

I. THE FLUCTUATIONS OF THE ATTENTION IN SOME OF THEIR PSYCHOLOGICAL RELATIONS.

By J. W. SLAUGHTER, A. B., B. D., Assistant in Psychology.

Since the first consideration of the fact of periodically varying intensity in the perception of minimal stimuli, no one has felt that any final word has been spoken. The matter was considered important as bearing on the ultimate theory of the attention during the historic discussion, beginning with Lange and ending with Pace, but afterward it was mentioned in the literature as one of the general facts of the attention compatible with almost any theory, all question of causality being left open. This state of affairs has a degree of justification if we remember that fluctuation is by no means the greatest phenomenon referred to when we speak of attention, a term covering a wide range of facts and of exceedingly indefinite connotation. If any one is so rash as to overreach in his inference, he will soon find his theory going to pieces before an advance along some seemingly unrelated line. So the cautious attitude is not to be entirely discouraged or distrusted. At the same time it is evident that any investigation of this kind must have as its ultimate purpose the elucidation of the most complex and hitherto most baffling problem in psychology, viz., that of the essential nature of the attention. Some are content to rest in general terms, as, for example, that attention is the activity of consciousness as a whole, basing their position upon some simple formula, as the law of relativity, without recognizing the fact that such a formula, perfectly sane as far as it goes, still goes only a little way toward elucidating the processes involved, until the limits and manner of its application are fully determined. The discussion referred to pivoted on a question arising from the analogy, useful for some purposes, but often misleading, which compares consciousness to the field of vision and makes a separation of the perceiving agent from the object perceived. Do the so-called fluctuations of the attention belong

to consciousness or to the contents of consciousness? Very few to-day would insist that this is a paramount question or that its answer would give a final solution to the problem of the attention. We may say that the attention is some form of interaction of the conscious contents, but various perplexing questions immediately arise. Is it not a mere figure of speech to speak of "contents" with a conjoined function of interaction? Is this interaction a process of reinforcement or inhibition or both? What physiological evidence can be brought in support of the view?

Experimental demonstrations, especially of a physiological kind, have come in very slowly in a field where they are most urgently needed. Still certain important additions to our knowledge of the physiology of the nervous system, coupled with modified views of the attention, which demand a basis in the facts of nerve function, render an orientation of the question unavoidable.

The facts to be dealt with are simple and familiar. If attention is given to any just noticeable stimulus, as the ticking of a watch held at the proper distance from the ear, the gray rings of the Masson disk, or slight electrical stimulation of the skin, a periodic variation between perceptibility and imperceptibility is noticed. These are the so-called fluctuations of the attention.

I.

Since the reviews of the literature bearing on the question are fragmentary and unequal, it will be of advantage to give a brief description of what has been done in this field. The phenomenon, already noticed by physiologists and physicists, was first systematically dealt with by Urbantschitsch in two investigations.[1]

In the first of these he found that when a clock is placed at such a distance from the ear that the ticking is just noticeable, a variation in the clearness of the sound, ranging from the distinct appearance of the separate strokes to entire disappearance, is perceptible, the transition being in some cases gradual, in others sudden. The same result was reached when the ear was closed and the sound transmitted through the bones of the head. On the basis of these facts, he concluded that the fluctuations have their seat in the acoustic nerve, which being subjected to continuous stimulation, soon becomes fatigued and recovers itself only after a certain period has elapsed. In the

[1] Ueber eine Eigenthümlichkeit der Schallemfindungen geringster Intensität. Centralblatt f. d. med. Wissensch, 1875. Ueber subjective Schwankungen der Intensität akustischer Empfindungen, Pflüger's Archiv. Bd. 27.

second investigation, Urbantschitsch experimented with the two ears at once and with subjects having defects of hearing, and reached the additional conclusion that the periods for the two ears do not coincide but alternate. The noise is noticed first on one side, then it seems to pass through the head and appears on the other side. In similar manner and with like results, Urbantschitsch investigated the other senses. If two points are placed on the skin at the "limen of twoness," the periods fall first above, then below, the limen. If the points are further apart, the fluctuation is between the two just as in the case of the two ears. If the two index-fingers are placed in hot water, the pain sensation is felt first in one, then in the other. He found that the same rule holds if two points of any sensory surface are affected by their specific stimulus.

The view of Urbantschitsch that the phenomenon depends on the periodic exhaustion and recovery of the sensory nerves, admittedly without support from what was known of nerve physiology, found an opponent in Nicolai Lange who investigated[1] the variations applying to them the name "fluctuations of the attention." His view was that the fluctuations were of central origin and depended in general upon the reinforcing function of the apperceptive activity. After establishing the periods as extremely short he proceeded to his crucial experiment which was to compare the periods found by applying minimal stimuli to disparate senses at the same time. It was found that the periods did not fall together but were separated by a definite interval. On the basis of this experiment, very questionable in itself because of the difficulty of attending simultaneously to disparate minimal stimuli, Lange drew the immediate conclusion that the fluctuations did not depend upon fatigue of the sensory nerves, as Urbantschitsch had thought, but must be referred back to the unitary activity of apperception. The first or negative part of this conclusion, as Eckener indicates, has a certain ground if we grant the accuracy of the experiment. The second part, as Münsterberg tells us with justification, throws the whole problem back upon an activity which, as Lange conceived it, lies partly or entirely within the region of the transcendental. Lange then proceeded to support his position with experiments on the familiar illusion of the steps and broken wall, finding that the fluctuations between the two fall approximately within the same time limits as those connected with minimal stimulation. Reasoning from the assumed fact that the change in the illusion is due to the varying power of reinforcement of the memory images of steps

[1] Beiträge zur Theorie der sinnlichen Aufmerksamkeit und der activen Apperception, Phil. Studien IV, pp. 390, ff.

and wall in the process of apperception, Lange concludes that some such varying memory images must lie at the basis of the fluctuations in the case of minimal stimulation. That this is a pure inference, unsupported by direct observation, may be seen by constant attention to any just noticeable stimulus, as the gray rings of the Masson disk, which during their disappearance leave nothing perceptible but the already present white background, showing that it is a case of appearance and disappearance and not of alternation. In general, one is inclined to favor the criticism which Münsterberg[1] passed upon Lange, "*. . . . . die Resultate seiner Experimente sind durch ihre überraschende Eleganz, durch ihre unerwartete Koustanz und Sicherheit, durch ihre leichte Verchmelzbarkecit mit metaphysichen Ansichten, einerseits geradezu prädisponiert zu wissenschaftlichen Dogmen zu werden, andererseits in hohem Mass den Verdacht nahe legend, dass irgendwelche Fehlerquellen übersehen worden sind.*"

The next investigation was that of Münsterberg[2] who took a position in opposition to that of Lange, asserting on the basis of his experiments that the phenomenon in question is to be referred to the fatigue and recovery of the accommodation muscles in the sense organs. This is the peripheral theory which is combated by nearly all the succeeding writers on the question. Münsterberg's investigation was limited to the sense of sight, on which he performed a series of elaborately varied experiments. His reason for choosing this sense department was, "Den Lichtreiz können wir unter sehr viel mannigfaltigeren Bedingungen darbieten als den Schallreiz. . . . . Wichtiger aber ist, dass wir das Auge auf beliebige Punkte richten und bewegen können." The method of experimentation was to fixate the gray rings of the Masson disk and record the fluctuations in series upon a white kymograph drum by means of a tambour and pencil, in this way improving upon the method of Lange, who measured single fluctuations upon the chronoscope. The general average of the normal fluctuations was first established as 6.9 seconds. The experiments were then varied by the use of a "prismatische Lorgnette," by which the field of vision was moved slightly to the side, requiring a quick movement of the eyes in order to keep the fixation continuous. After some practice the subject could make the necessary movement so quickly that the interruption was scarcely perceptible. In case the prisms were held continuously before the eyes naturally no very great change in the fluctuations would enter. But when the glass was inter-

[1] Beiträge zur experimentellen Psychologie, Heft II, p. 78.
[2] Die Schwankungen der Aufmerksamkeit, Beiträge II, pp. 69, ff.

posed at intervals of two seconds, it was found that the fluctuations could be lengthened to 11 to 14 seconds. The average was 12.3 with an average variation of 3.1 seconds. In the next series, a sound was made by an assistant every second, which caused the subject to close the eyelids quickly for a moment, making a scarcely noticeable interruption in the fixation. "A decrease to entire vanishing never took place." A quick voluntary closing of the eyes every two seconds produced the same result. The next variation was by the interposition of a gray covering which for a short interval completely hid the disk from view. "The effect is now entirely different, . . the vanishing appears much oftener than by normal, uninterrupted fixation." The average length of the fluctuations was 5.8 as against the normal 6.9 seconds. The same general results were reached by making other variations, such as the use of indirect vision, and moving the whole apparatus slowly in different directions. In the last series of experiments, Münsterberg investigated the connection between breathing and the fluctuations. When the respiration was in short gasps a distinct shortening of the fluctuations was noticed; a lengthened respiration gave a corresponding lengthening of the fluctuations. In the latter case there was often a direct correspondence between inspiration and vanishing, but not seldom the two proceeded with entire irregularity.

Münsterberg's theoretical conclusions will concern us here for only a moment as they will be noticed later on. Realizing that the intervention of a transcendental function of consciousness never suffices for a scientific explanation, he decided that the variation must lie in the region of "contents," *i. e.*, must have a peripheral origin. He accounts for the process in this way. The gray rings of the disk, standing out only in the slightest degree from the background, require exact accommodation and fixation. Any cause, either artificial or of the nature of fatigue, which produces a change in the tension of the muscles, necessarily renders impossible for the time being the perception of the rings.

In opposition to the view of Münsterberg appeared a series of articles, the first of which was by Eckener.[1] This investigation is important as bringing the apperceptive factor in the process strictly within the lines of scientific explanation. Münsterberg had practically admitted the transcendental element in making the separation between consciousness and the conscious contents, the first being in the process under consideration a fixed will to attend, a complex of numerous motives,

[1] Untersuchungen über die Schwankungen der Auffassung minimaler Sinnesreize. Hugo Eckener, Phil. Studien, VIII, p. 343.

etc., the second being the act of perception itself. Eckener in a rather rigid criticism shows the impossibility of this distinction.

What Eckener does, beyond the work of Münsterberg, is to extend the method of investigation to other sense-departments, and on the basis of these results and a careful introspective analysis of the conditions involved, draw the general conclusion that all the causal factors are of a central nature. In particular, he points out the close connection between the ease with which the memory-image of a sensation is kept in clear consciousness and the fluctuation of the sensation itself. The conclusion from this is that the general psychophysical condition which connects the memory-image with the actual process of stimulation must vary in some way before the fluctuations can appear. The causes of such a variation are not far to seek. They lie partly within the nature of consciousness itself as an organization of dynamic not static elements, partly in the activity of other sensations claiming a share of the attention. In other words, the reason for the fluctuations lies in the familiar phenomenon of distraction. The criticism we will here pass upon Eckener is that he states the conditions of the problem without giving a real solution. Suppose we grant that the process is of a central nature, how much nearer are we to an intelligible understanding of its real nature? The term *apperception*, as he understands it, has no clearly definable laws, and, until we can determine them, it is absurd to apply it for purposes of scientific explanation.

Pace, in a companion article[1] to that of Eckener, describes an experimental test of Münsterberg's conclusion that the fluctuations depend upon variations in visual accommodation. Working only with the Masson disk, he first establishes the averages for the normal vision of his subjects, then paralyzes the ciliary muscles by the use of atropin, and finds that, with the power of accommodation entirely lacking, the fluctuations proceed with only a slight variation from the normal. The obvious conclusion is that the essential conditions are central. The slight variation would indicate some kind of reciprocal action between center and sense organ, but as to the nature of this he gives no opinion.

In the investigation of Marbe,[2] carried on without knowledge of what was being done by Eckener and Pace, the position of Münsterberg is attacked from another side. "Die Theorie Münsterberg's . . . . ist nur haltbar wenn drei Voraussetzungen

[1] Zur Frage der Schwankungen der Aufmerksamkeit. Phil. Stud. VIII, p. 388.

[2] Die Schwankungen der Gesichtsempfindungen. Phil. Stud. VIII, p. 615.

erfüdlt sind: wenn nämlich erstens die Schwankungen nur bei ebenmerklichen Reizen eintreten; wenn zweitens die Reize, damit sie überhaupt sichtbar werden, exacte Accommodation und Fixation erfordern, wenn drittens die Schwankungen nur bei dunkeln Punkten auf hellen Grund stattfinden." On this basis, a few simple variations in the method of experimentation are sufficient to overthrow the opponent's position. What Marbe really does is to determine the fact that when the intensity of the stimulus is varied within the very narrow possible limits, in the case of visual sensations by changing the degree of brightness or the distance from the eye, a ratio is found between the degree of variation and the length of the fluctuations. With a stimulus of greater intensity or nearer the eye, the periods of disappearance are shorter. We shall refer to this again.

In the investigation of Lehmann,[1] the theories of the former writers are rejected as one-sided, and an attempt is made to analyze some of the physiological factors in the process. Following a suggestion made but rejected as improbable by Münsterberg, he studies by a simple experimental arrangement the relation of breathing to the fluctuation. In the case of slight electrical stimulation, it is found immediately that the curves representing the respiration and the course of fluctuations, are in large measure coincident. This is not so evident in the case of sound or light stimulation, but, by plotting the averages of a large number of experiments on co-ordinates divided to represent the different stages of a respiration, the general result is determined that the maxima of the fluctuations fall near the highest point of the inspiration. To account for the irregularity, Lehmann brings in other factors, in the case of sound, the related memory-image determined by Eckener, in the case of light, both the memory-image and Münsterberg's variation in accommodation. As to the precise way in which the respiration affects the fluctuations, we are left in some doubt. ". . . . die Reactionen sind am haüfigsten in der Nähe des Inspirationsmaximums. Hier ist eben der Blutdruck am grössten, und von diesem Zustand muss angenommem werden, dass er für die psychophysische Arbeit des Gehirns günstig sei. Wir wissen ja, dass das Blut, während der Arbeit irgend eines Organes, demselber reichlicher zufliesst." No one will dispute the fact that any organ in activity, by reflex excitation of the vaso-motor center, receives a larger supply of blood. This is eminently true of the brain. But that the activity of the muscles of respiration should cause

[1] Ueber die Beziehung zwischen Athmung und Aufmerksamkeit. Phil. Stud. IX, p. 66.

a greater flow of blood to the brain does not appear from this process of reasoning.

The valuable investigation of W. Heinrich[1] regarding the influence of central processes upon the activity of the visual sense organ, contains two results of importance to our consideration.

(a) "Wird die Aufmerksamkeit nicht-optischen Eindrücken zugewendet, so wird das Auge akkommodationslos, es kann sogar eine noch stärkere Abflachung der Linse eintreten, wie beim Fernsehen."

(b) "Wird die Aufmerksamkeit von den optischen Eindrücken abgewendet, so ändert sich die Konvergenz der Augenaxen. Diese nähern sich der Parallelstellung."

Heinrich proceeds without critical examination to accept the peripheral explanation of the fluctuations, not recognizing the fact that, on Münsterberg's ground, so soon as the central factor is admitted, this interpretation is no longer possible.

This review of the literature affords us a basis for distinguishing three views regarding the fluctuations: that they have a central cause, that they have a peripheral cause, and that they have a purely physiological cause. Each of these is open to certain criticisms.

(1) The school of Wundt finds itself in the unfortunate position of being committed to the interests of a definite theory. This theory, notwithstanding the fact of its great utility, is still so general that facts referred to it are not really explained, and, like all general theories, is easily disturbed by any apparently antagonistic fact. The consequence is that its supporters approach every question with a certain degree of preconception, which shows itself in the present case. (a) The variation must be in the clearness and not in the intensity of the sensation. (b) The falling away from clear consciousness must be due to interference and inhibition by other conscious elements. (c) The explanation must be predominantly psychological. This last is a basis of a very valid objection. If the phenomenon is to be viewed purely from the standpoint of apperception, there is nothing to determine its periodicity but the nature of the conscious elements involved. These are admittedly of very unequal importance in consciousness. On the other hand, the fluctuations, in spite of their variations, show a significant degree of regularity. This would predispose one immediately to look for some rhythmical physiological process as a basis of explanation.

The peripheral theory of Münsterberg, on the other hand, has the advantage of being at first sight very simple and plausible. But to grant that the fluctuations depend entirely upon

[1] Die Aufmerksamkeit und die Funktion der Sinnesorgane. Zeitschrift für Psych. u. Phys. d. S., IX, p. 342.

fatigue in the accommodation muscles of the sense organ, is to grant the previous assumption that this mechanism is relatively independent of the central factors. No such isolation seems possible. First, muscular fatigue, after the researches of Mosso and Lombard, must be interpreted as exhaustion of the innervating center. If it is replied that this is a purely reflex process, then, secondly, we may adduce the well known fact of central control of reflexes. This was definitely proved in the case of the eye by the investigation of Heinrich noted above. The primary impulse must come from the central organs. Further the theory may be questioned on the basis of fact, as the results of Pace's experiments show. Our own work on Dr. O., mentioned below, enters in as a definite disproof.

A theory like that of Lehmann, besides being very indefinite in itself, describes processes which have no psychical counterpart, so, however useful it may be in other directions, does not lend itself to the explanation of a psychological phenomenon.

II.

Our experimental work took, as its point of departure, two facts derived from former investigations: (a) that the weight of evidence lay decidedly on the side of a central causal process of some kind; (b) that the general theories of the attention and apperception required physiological supplementation before they could give an adequate explanation of the phenomenon. This gave a determinate direction to the work. It was necessary,

1. To make an accurate, detailed study of the fluctuations in order to determine any characteristics manifested in their actual process of occurrence.

2. To study the connection between these characteristics and any physiological processes that might be found to influence them.

The experiments were begun in the fall of 1898, and continued for the greater part of two years. The following persons acted as subjects for all or part of the experiments: Prof. Pillsbury (P), Miss Earhart (E), Messrs. Vought (V), Kirtland (K), Stevens (St), Dr. Oliver (O), and the writer (S). All of these except O. had had considerable training in psychological methods of work. None of them except Prof. Pillsbury and the writer had any knowledge as to the purpose of the investigation. Each subject was given several days' training before his results were treated as trustworthy.[1]

[1] Perhaps no kind of reactions are at first more indefinite than those in this field. The object obtained by the preliminary training was the reflex registration of *all* the fluctuations and the exact boundaries of each. The best test of adequate preparation was the subject's own feeling of satisfaction with his reactions.

In order to carry out the first of the objects indicated above, it was necessary to obtain the fluctuations in series of suitable length. For this purpose a long horizontal drum was used, covered as usual in registration experiments with smoked paper. A small stand, bearing the Marey tambours and electrical markers, was moved along the drum by a threaded rod, turning uniformly with the revolutions of the drum. In this way a continuous series, lasting as long as ten minutes, could be secured. The tambour recording the fluctuations, was connected by a rubber tube with another tambour provided with a pressure-key which was operated by the finger of the subject. Time was recorded by a Jacquet-Verdin clock, indicating fifths of a second. The minimal stimuli used in nearly all the experiments were the gray rings of the Masson disk, as they were found to be most suitable for continuous series, as well as for furnishing the most unambiguous results. The disk used was of white cardboard and 26 cm. in diameter. Along one of the radii was a series of carefully outlined black spots, each of which was 4 mm. square. The distance of the spots from each other was likewise 4 mm. As the daylight was found to be exceedingly variable for the work, the reflected light of an incandescent lamp, carefully shaded from other parts of the room, was allowed to fall upon the disk. The distance of the subject from the disk was kept uniformly at one and one-half meters.

1. Normal Fluctuations.

The average time-lengths for the different subjects and different series were found to vary within such wide limits, that our original intention to make the individual fluctuations as occurring in a series the object of study, soon found sufficient justification. The time values were plotted directly on cross-section paper, a millimeter to the fifth of a second, those representing the visibility of the stimulus being above, those representing the non-visibility, below the horizontal axis. Distance was, of course, allowed on this axis for the lengths of both, reduced, however, to seconds. The general results will be evident from the following curve. The numbers indicate full seconds.

This curve is taken from the very earliest series, but the same characteristics are shown throughout. The characteristics are evident.

(1) The curves representing the visibility and the non-visibility of the stimulus do not remain constant but vary in approximately definite periods of two kinds, the first from 10 to 15 seconds, the second from 60 to 80 seconds in length.

(2) These variations seem, in a measure, independent of the number of fluctuations.

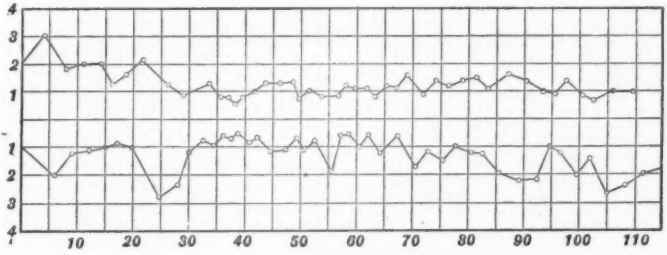


FIG. 1. SUBJECT P.

(3) The periods of visibility and non-visibility vary their length with reference to each other not in inverse but in direct proportion.

(4) In general, as the curves approach each other, that is, as the periods both of visibility and non-visibility shorten, the former seems to be relatively greater in length than the latter.

These generalizations are based upon the results of about forty series, of which the short section reproduced is considered representative. While these data afford very meager evidence for any kind of general conclusion, still they give certain suggestions which it may be pertinent to indicate.

(a) The fact that we find in addition to the primary fluctuations a secondary and a tertiary period regularly recurrent and largely independent of the number of fluctuations, would suggest that we are dealing with a composite process, showing the influence of several distinct physiological rhythms.

(b) The fact that a period of visibility is attended by a proportionally long period of non-visibility suggests the erroneousness of the commonly accepted view that the length of time a stimulus is effective in consciousness is a direct measure of the efficiency of the attention. If we can assume that the ratio of attention-efficiency is the ratio of the preponderance of the time-lengths of visibility, the converse seems to be the case. The most effective attention is attended by short and therefore rapidly recurring fluctuations. This position is sustained by the investigation of Taylor carried on in the Michigan Laboratory the past year. The experiments were sufficient at any rate to confirm our original opinion that the fluctuations are of central origin but incapable of explanation on a purely psychological basis, moreover, that the investigation of the physiological processes involved would be an analysis of the groundwork of the attention.

2. Effect of Voluntary Effort.

It is a well known fact that the efficiency of voluntary effort in any part of the body is increased if attended by strain in other parts. The only possible explanation of this seems to be that the efficient discharge of a group of motor cells is in direct proportion to the extent of area excited. Or, in other words, the impulse from the motor cell is not only transmitted along the regular path for the innervation of the muscle, but is diffused to the cells of other motor parts of the cortex. Can a like diffusion extend to sensory cells? If the general law is established, it will be a comparatively easy matter to show how the centers controlling the rhythmical functions of the body exert the influence on the course of visual fluctuations already indicated.

To study this effect the same arrangement was continued as before, except that another tambour was added connected by a rubber tube with a Verdin dynamograph.[1] In the later experiments a Cattell spring ergograph provided with a tambour was substituted for the dynamograph. The course of fluctuations was taken normally as before for the first half of the series, then, at a word from the experimentor, pressure was exerted upon the instrument by the left hand of the subject. Of course the strain could not be continued for any great length of time, so the recording pointer gradually came to its first position. One fact that may be noted in this connection is that the pointer invariably fell during a period of non-visibility, the effort being always sustained during the period of visibility. The general results of the method will be evident from the following figure.

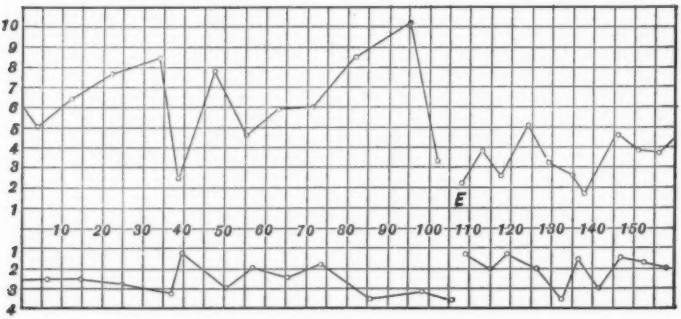


Fig. 2. Made on Subject K.

The figures indicate full seconds. At point marked E., strain upon ergograph is begun.

The fact which this figure is intended to illustrate is obvious. The exertion of pressure upon the ergograph by the subject is followed by an immediate and distinct shortening of the periods both of visibility and non-visibility. An additional fact, not so clear from the figure but established by all the averages made by the writer, is the relative increase in the periods of visibility over those of non-visibility. Adding the remainder of the ergograph series to the figure shown, and averaging, we get the following Table.

TABLE I.

Subject K.

	N.	Av. F.	Av. V.	Av. UV.	Ratio of V to UV.
Normal	13	45	31.9	13.1	2.43
With Ergo.	28	33.4	24	9.4	2.55

The Table gives the complete series of which Fig. 3 is a part. The following Table shows the same results for another subject.

TABLE II.

Subject V.

	N.	Av. F.	Av. V.	Av. UV.	Ratio of V to UV.
Normal	21	29	16.5	12.5	1.32
With Egro.	28	23.4	13.9	9.5	1.46

N represents the number of fluctuations in the series, *F*, the value of the whole fluctuation, *V* and *VV*, the periods of visibility and non-visibility respectively. The last column shows the increased value of the periods of visibility in the ergograph series. The numbers indicate fifths of seconds.

Compare the above Tables with the following one taken from Marbe's investigation.

TABLE III.

(From Marbe.[1]*)*

Subject.	Distance from Obj. in CM.	Av. F.	Av. Intermission Period.	Av. Sensation Period.	Inter. into Sensation.
G. M.	370	7.8[2]	2.7	5.1	1.9
G. M.	483	9.0	4.5	4.5	1.0
K. M.	215	11.9	2.1	9.8	4.7
K. M.	425	17.4	10.2	7.2	0.7

[1] *Op. cit.* p. 623.
[2] The time here is in seconds.

What Marbe designs to show by the Table is that the efficient perception of the stimulus varies with the distance of the object. He found the same true with variations in the brightness of the object, and summed up his conclusions in the formula that the fluctuation lengths are a function of the intensity of the stimulus. A comparison of the two Tables will show that our results are in close agreement with his except that in the ergograph series the periods of visibility are also shortened. As to the main point, as shown in the last columns, there is substantial coincidence. The conclusion from this is that muscular effort has the same effect on the fluctuations as an increase in the intensity of the stimulus.

How is this to be explained? Without anticipating more general theoretical conclusions, it may be said that the probable explanation is the same as that of the increase in the efficiency o a motor activity by other motor activities. In other words, that impulses emanating from motor cells act upon sensory cells by way of increasing their excitation. It is probable that some of Münsterberg's results, *e. g.*, the shortened fluctuations with rapid breathing, and the shortened periods of non-visibility with eye movements and quick closing of the lids, can be explained on the same basis as being due to the exertion of effort. Another apparent effect was that the subject could see the fainter rings at the periphery of the disk during the dynamograph or ergograph series, which were invisible during the normal series. This cannot be stated positively as it might have involved an element of suggestion.

An additional fact that may be noted in this connection is that the increased efficiency shown in the ergograph series is the same as the lowered limen of sensibility under the condition of maximal attention. The close connection between maximal active attention and strain sensations resulting from motor activity, hardly needs to be indicated. The bearing of the general questions of distraction and apperception upon what has been said, will be noticed in a later connection.

3. Relation to Vaso-motor Periods.

The investigation to this point has been concerned with what seems to be merely extraneous influences upon the course of fluctuations. The question now arises as to how the fluctuations themselves are to be explained. Is the process that causes them different in kind from the processes that influence their variable lengths? To pass over now to the purely psychological view of apperception would be a violation of the common psychophysical assumption of parallelism. Our further experiments answer the above question in the negative.

At this time an investigation[1] was being carried on in the physiological laboratory by Professors Lombard and Pillsbury relative to the changes in the rate of beating of the normal human heart. Distinct periodic changes in the pulse rate were made out, which were found to stand in constant relation partly to the respiration and partly to the Traube-Hering waves. The only possible explanation seemed to be that various impulses notably from the vaso-constrictor center flow over and act upon the vagus center, the effect showing itself immediately in an inhibition of the control apparatus and consequent accelerated action of the heart.

These experiments called our attention to the approximate equivalence in time existing between the fluctuations and changes in heart-rate, and at the same time showed a type of nerve activity that might possibly be a means of explanation. Since the changes had, as indicated, been traced to vaso-motor activity, it was determined to try the fluctuation series in connection with the vaso-motor changes. For this purpose it was necessary to alter slightly the arrangement of the apparatus. The curves were registered on the vertical drum of an ordinary kymograph, driven by clockwork. The rate of revolution was adjusted so that the fifths of seconds indicated by the time-marker were just distinguishable. This slow movement required a sharply outlined registration of the beginning and end of each fluctuation, so, instead of the tambour and pressure-key, an electrical marker was used. An ordinary telegraph key served to make and break the current. The various changes in blood-pressure were registered by means of a delicate piston-recorder[2] connected with a finger-plethysmograph. The point of the piston-recorder was kept on an exact vertical line with those of the time-marker and the marker indicating the fluctuations. The plethysmograph was surrounded by a water-jacket through which a stream of water of constant temperature was passed. This arrangement served to register the pulse, the respiration periods, the Traube-Hering waves, and all other changes in volume. The record varied with the different subjects, and with the same subjects under different circumstances. At one time the pulse only would be distinguishable, at another time, the respiration or Traube-Hering waves, so that it was only under the most favorable circumstances that a record of all the changes could be secured at once. The pointers were placed on the same line, and a direct comparison of the parallel tracings was possible. Attention should be called to the fact that this method of experi-

[1] American Jour. of Phys., Vol. VII, p. 201.

[2] For a description of this instrument and its uses, see Amer. Jour. of Phys., Vol. VII, p. 186.

menting made it quite impossible for any predisposition on the part of the subject to influence the validity of the results.

The tracings reproduced in Figure 3 will illustrate the method and the result reached by it. The upper curve indicates time in seconds; the middle one, the series of fluctuations, the raised parts corresponding to the periods of visibility; the lower shows the changes in blood-pressure, only the pulse and Traube-Hering waves being distinguishable.

Fig. 3. On S.

The coincidence of the periods of visibility and those of increased blood-pressure in the finger is immediately evident. This was substantiated on subjects P. and S. by about fifty series on each. The reason for such an extensive experimental test is that it was exceedingly difficult to keep the circulatory conditions sufficiently uniform to obtain a continuous series of vaso-motor effects. The lever of the delicate piston-recorder was lowered with the slightest variation in conditions of sound or light, or the intrusion of any foreign idea into the subject's consciousness. In estimating the results, only those sections of the tracings showing distinct waves for five or more successive periods, were treated as evidence. The following method was observed in reading the tracings. The plethysmograph curve was levelled up to a straight line directly under the fluctuation curve, by means of dividers separated to the exact length of the piston-recorder lever. This made it possible to compare directly the two curves with a view to determining their coincidence. A case was treated as one of coincidence when the period of visibility began during the rise of one blood-pressure wave and ended before the beginning of the next. Every case in which the period of visibility began during the decline or reached over to the next, or in which there was a fluctuation without a corresponding blood-pressure wave or *vice versa*, was treated as a contrary case. The general estimation of the readings, made under the above restrictions, shows the following rate of coincidence as compared with all the cases:

For subject P., 83% in 106 cases.

For subject S., 90¾% in 258 cases.

The presence of even these few contrary cases might throw doubt upon any conclusion based upon the results, but it must be remembered that there are many sources of interference quite beyond experimental control. There is always the possibility of a slight inaccuracy in the subject's registration of the fluctuations. Again, as we have already seen reason to believe, there are probably other physiological processes that exert an influence upon the length of the periods.

In the course of our experiments there appeared, in addition

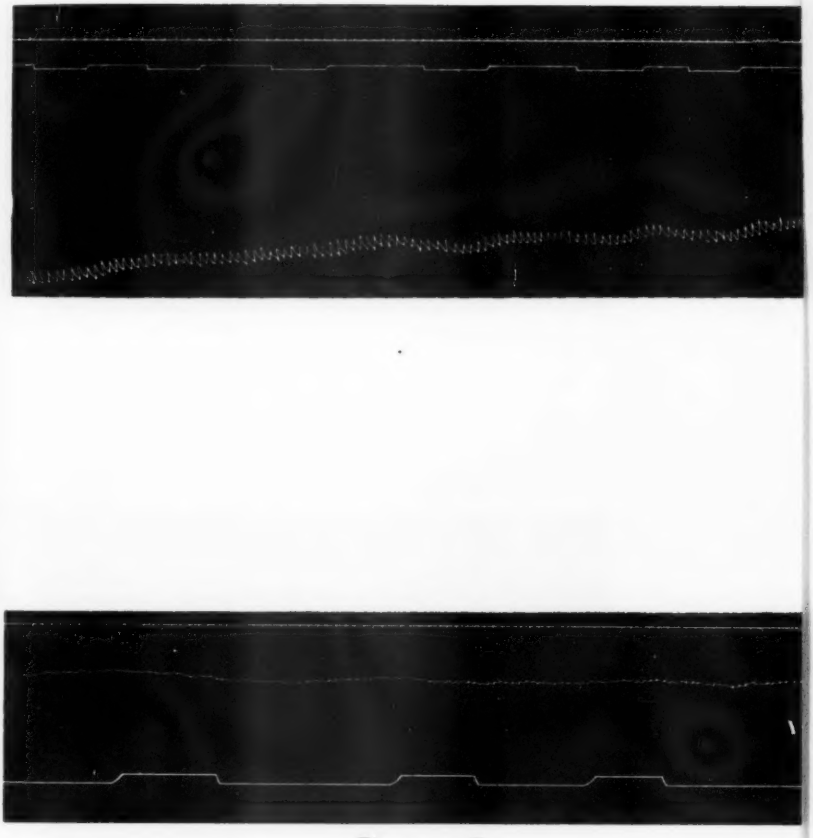


Fig. 4 on E.

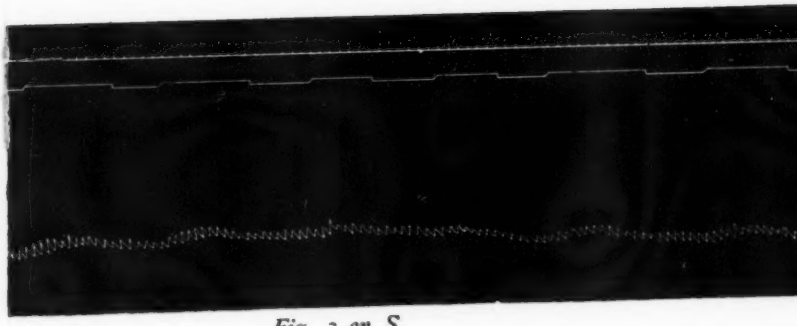


Fig. 3 on S.

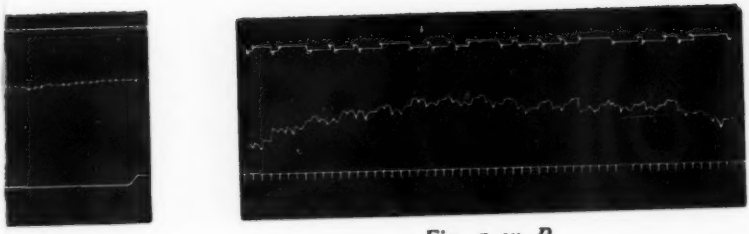
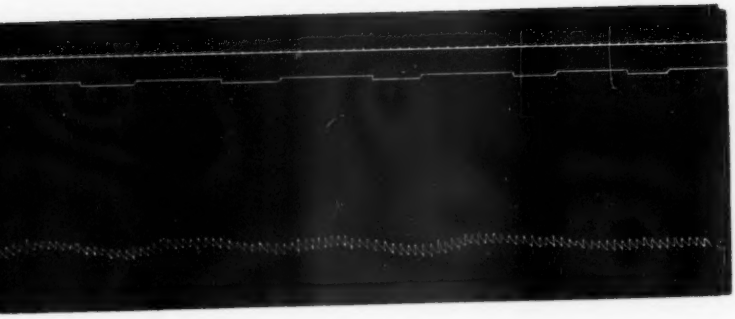


Fig. 5 on P.



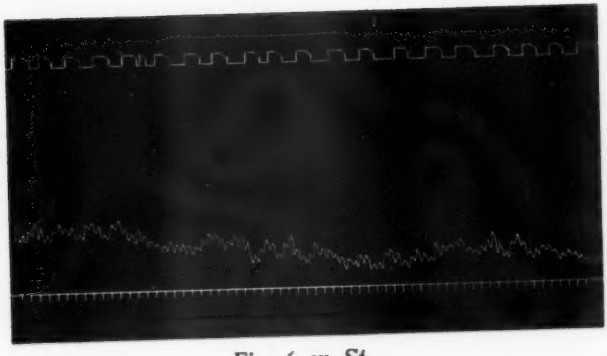


Fig. 6 on St.

to those mentioned, a long plethysmographic wave, the origin of which could not be accounted for. This came out only under the most favorable circumstances, and was very susceptible to any kind of disturbance. The measurement of these showed an average of 44 seconds, with an average variation of 4.25 sec. There is some reason for supposing that there is a connection between the variations in fluctuation lengths noted in Sec. 2 and these long waves, but the difficulty of obtaining a number of the latter in series made us waive for the time being the experimental test of the point.[1]

The peculiarity of one of the subjects, *E.*, should be noted. From the first extraordinarily long fluctuations were given, the periods averaging 21 sec. with an average variation of 3.16 sec.

Fig. 4. On E.

The introspective record showed the reliability of the subject's judgments as to lengths. When later the plethysmograph was used, it seemed impossible to obtain even a pulse tracing. By increasing the temperature of the room and the water in the jacket, some of the waves were brought out. The pulse was never visible, the respiration wave never came out very clearly, and there was an exceedingly long vaso-motor wave, only slightly discernible. This latter coincided very closely with the long fluctuation mentioned.

The discussion of the theoretical bearings of this section will be taken up in the conclusion.

4. Relation to Breathing.

The fact was noticed early by several of the subjects that if, during the period of disappearance, an especial effort was made to recover the stimulus, the rings would flash out for a moment and immediately disappear. This was undoubtedly the source of the indefiniteness and confusion experienced by the subjects during their preparation. The training directed their attention to the longer fluctuations, and gradually made the momentary flashes imperceptible. Near the close of the experimental work, *P.*, being the most thoroughly trained subject, began to

Fig. 5. On P.

look for these and register them together with the longer fluctuations. No cause for their existence could be ascertained until the pneumograph was tried, when they were found to

[1] Howell and Shields found similar long plethysmographic waves but of greater length in their work on sleep, but so far as the writer knows no physiological explanation has been offered for them. Journal of Exp. Med., Vol. II, pp. 325-326.

follow accurately the periods of respiration. Then a subject was tried, who had been trained before but not used on account of the extreme shortness and constancy of his fluctuations. Only the plethysmograph was used, on account of the possible suggestion that might be given by the pneumograph. The result showed a strong pulse tracing and an extremely marked respiration wave. There was little or no indication of the Traube-Hering waves. The fluctuation periods corresponded closely with those of respiration. There were at in-

Fig. 6. On St.

tervals breaks in the correspondence, but this is what would be expected from the interference of the longer period. When the subject was questioned at the close of the work, he stated that he had been perfectly unconscious of the respiration process, that a connection between breathing and the fluctuations had never occurred to him.

The results here are in agreement with those of Lehmann who found in the case of minimal electrical stimulation an almost perfect correspondence between the fluctuation and respiration periods. The ambiguity in his method of explanation has already been indicated.

One important result to which this section brings us, is the complete uselessness of attempting to establish an absolute value for the fluctuations. The physiological influences that control them seem to differ widely with the various subjects and for the same subject at different times. This probably also accounts for the disparity in time values obtained from different sense departments.

The general results of our experiments would show, then, that the fluctuations of the attention are in close connection with at least three physiological rhythms. That in most subjects they run parallel to the Traube-Hering waves of blood pressure, as Exner has suggested might be possible.[1] That in some subjects the breathing undoubtedly plays the predominant role, and that in one subject it is probable that there is still another longer wave that assumes the more important place. Furthermore, even in the subjects whose main rhythm corresponds to the Traube-Hering waves the other influences are not entirely lacking. As Lehmann found, and his results are confirmed by the work of Taylor, even the longer attention waves tend to change their direction at a definite point in the respiration rhythm. Again, careful observation shows that the breathing rhythm continues to bring out the gray rings during the time occupied by the trough of the Traube-Hering wave.

[1]Exner: Entwurf zu einer physiologischen Erkärung der psychischen Erscheinungen, p. 303.

On the other hand the longer waves, whatever their origin may be, betray their influence in the varying lengths of the fluctuations corresponding to the Traube-Hering waves.

The interaction of these varying influences, now one, now another predominating in the different individuals and in the same individual at different times, would account for the variations in length, the limits of which have been set by Pace as 3 and 24 seconds.

5. The Case of O.

The present section has little connection with the above series of experiments, but should be appended as the description of an interesting case in connection with the old discussion as to the origin of the fluctuations. *O* was troubled by cataract in both eyes. In an operation taking place in July, 1890, the lens of the right eye was punctured, resulting in degeneration. The entire lens was removed in two subsequent operations. The same series of operations had been begun upon the left eye which, however, is not yet effective for seeing.

Owing to circumstances it was impossible to give much preliminary training, or take a very extensive series of experiments, so the reproduction of figures will have only a relative value. The averages found are:

Whole fluctuation, 6.72 sec.

Period of visibility, 4.89 sec.

Period of non-visibility, 1.83 sec.

The main point of interest is that the distinct appearance and disappearance of the gray rings took place just as with a person of normal vision. With the absence of the power both of convergence and accommodation, the result bears conclusively upon the peripheral theory of Münsterberg, and especially upon Heinrich's rather dogmatic support of it.[1] It again possesses a decided advantage over the results of Pace as he could never be absolutely certain that the atropin had entirely destroyed the power of accommodation.

III.

That nerve cells interact in some way, and that the activity of large groups of them can be focussed in definite directions, is an assumption that lies at the very basis of all psychological investigation. So long as no question as to how this activity goes on is raised, the assumption is granted without contradiction. The existence of the suitable anatomical structures is sufficient evidence that it takes place. One definitely proved physiological fact would go far toward settling psychological

[1] Die moderne physiologische Psychologie in Deutschland, pp. 125, ff.

disputes as to reinforcement and inhibition. There are, to be sure, a few special kinds of nerve activity which are intelligible to the physiologist. For example, (a) the transmission of sensory and motor impulses from one neuron to another; (b) the inhibitory control of certain reflexes such as those of micturition, parturition and defecation; (c) the flowing over of impulses from a sensory to a motor center, such as the fact discovered by Schäfer that stimulation of parts of the occipital and temporal cortex is followed by definite movements of the eye.[1] Investigations throwing light upon the influence of psychical states upon nervous activity are scarce but not entirely wanting. For example, Lombard has shown that the knee-jerk is reinforced by attention, mental work, music, etc.[2]

Our own investigation of the vaso-motor processes and voluntary effort, cannot be said to prove conclusively the additional fact that the activity of motor cells reinforces the activity of sensory cells, because one of our terms was a psychical state probably involving a number of factors, but the connection between this state, primarily bound up with a sensory process, and the motor activities mentioned is unmistakable. It might be argued regarding the connection with the vaso-motor process that the increased efficiency of the attention during the period of visibility is due to the increase in blood supply. This is the position of Lehmann, in the way of which there are certain very definite difficulties. In the first place, Lehmann takes as the basis of his theory the fact that when any organ is active it receives a larger supply of blood. From this he immediately concludes that the rhythmical respiration process alters the supply of blood to the brain. All that can be concluded from the premise is that the alteration is in the supply of blood to the respiratory organs. In the interval it may go to the brain or may not. Again, on his basis the impulse which serves as a demand for increased supply must originate in the activity of the organ itself. Consequently the periodic presence of a larger amount in the brain assumes a *prior* periodic increase in brain function, which puts the problem further back. In the third place, without attempting to dogmatize on the matter, it is almost safe to say that the rhythmical Traube-Hering waves, with which we found the fluctuations connected, are not the result of a specific demand from any part of the body. When such demand is made, the registration of these waves is interfered with, as was seen in our experiments in the instant fall of the lever when effort of any kind on the part of the subject took place.

[1] Proceedings of the Royal Society, 1888, Vol. XLVIII.
[2] *Amer. Jour. of Psy.*, I, 1.

One other question stands in the way of our making a definite affirmation in regard to vaso-motor activity, viz., the unsettled difference between the positions of Mosso[1] and Howell concerning contrary circulation. If the supply of blood in the brain is the reverse of that in the body, then obviously in our case some cause for the fluctuations other than the presence of blood must be looked for. On the other hand, if the circulation is direct in both body and brain, the blood supply factor may enter in. But granted that the latter view is correct, the question still remains as to whether the presence of more blood has any effect whatever upon the function of nerve cells.

The phenomenon seems most explicable when put on the same basis as the fact that respiration waves are found in blood-pressure tracings, and that both respiration and vaso-motor effects are found in the rate of the heart. That is, the two centers, controlling functions of enormous importance and extent, must originate correspondingly powerful impulses, and these impulses not only proceed along their regular paths but overflow to other centers and are transmitted to the cortex, there acting by way of added excitation upon groups of sensory cells. The question might be raised as to whether, granting an influence of some kind, the impulses reinforce the process of stimulation or add to the energy of the cell. The question, however, is not important, for us the significant point being that *they in some way reinforce the functional activity of sensory cells*. We have already indicated the fact that sensory stimulation has the same effect upon the fluctuations as voluntary effort. So our general conclusion would be that the activity of any group of cells is not only a direct response to the stimulus, but is in large measure dependent for its efficiency upon the reinforcing influence of other groups of cells.

An objection to this position may be based upon the question of distraction. According to the Wundtian position, distraction, as seen in the apperceptive struggle of ideas, lies at the basis of the fluctuations. Eckener, as the exponent of this position, gives a long list of the possible influences claiming attention. Prominent among these are mentioned the sensations arising from the general stimulation of the sense organs disregarded for the time being. The objection is answered by the investigations of Miss Hamlin[2] and Münsterberg,[3] who showed that these influences, so far from being detrimental to the activity of the attention, are really necessary to its highest efficiency.

These results are in entire accordance with the type of ner-

[1] Ueber den Kreislauf des Blutes im menschlichen Gehirn, 1881.
[2] *Amer. Jour. of Psy.*, VIII, p. 1.
[3] Psy. Rev., I, pp. 39, ff.

vous activity to which our experiments on the whole seem to point. They further seem to furnish the best method by which to approach the general question of apperception and the mode of its activity. The view which regards the apperceptive relations of ideas as based entirely upon inhibition, is founded upon a kind of mental mechanics not at all consonant with the facts of nerve physiology. Inhibition is certainly one of the functions of the nervous system, but the word is used with reference to the effect and not the process. Nor is the position in agreement with the results of the investigations of Münsterberg and Miss Hamlin just mentioned. We naturally deal with the question from the point of view of our own experiments. While our results are not sufficient to warrant the final affirmation of a theory of reinforcement, still the facts regarding nerve function, the basis most needed for any kind of theory, point decidedly in that direction. The affirmation of the importance of this side has been definitely made by Exner and others of prominence. The view makes most intelligible the fact of unified interactivity of conscious elements, and at the same time saves us from the difficulties incident to regarding the attention as a special process.

Summary.

The results of the investigation, stated generally, are:

1. The fluctuations of the attention do not depend, as formerly affirmed, upon either the apperceptive process or changes in the sense organ.

2. The periods do not remain constant but have a definite order of variation.

3. Voluntary effort shortens the fluctuations and increases the relative efficiency of the attention.

4. The periods stand in close relation with the vaso-motor and respiration processes.

5. The causal process is physiological in nature, and probably acts by way of a reinforcement of the activity of the nerve cell, not indirectly through changes in nourishment, due to variations in blood pressure.

II.

THE EFFECT OF CERTAIN STIMULI UPON THE ATTENTION WAVE.

By R. W. Taylor, M. A.

The dominant place of attention in consciousness gives great interest to all experiments along that line. This paper deals with one of the minor phases of the duration of the attention. Our more particular problem was to investigate the influence of certain stimuli upon the length of the attention waves in the hope that the results would throw some light upon the much disputed question as to whether the waves were of central or of peripheral origin.

The Masson disk was used to give the minimal stimulus necessary, as it is at once more easily manipulated and the results obtained are less likely to be influenced by distracting stimuli than if a minimal sound or pressure is chosen. In these experiments the disk used was 32½ cm. in diameter, and had drawn upon one radius 15 dots, each 5 mm. long, 4 mm. wide, and separated from each other by spaces of 5 mm. The fluctuations were recorded by a Marey tambour upon the horizontal drum described by Slaughter.[1] At the rate used a single record lasted 8¾ minutes. The time was recorded in fifths of seconds by a Jacquet-Verdin chronograph. The reagent sat at a distance of two meters from the disk. Pressure upon the receiving tambour marked the time of appearance of the gray lines, and relaxation of pressure the time of disappearance.

The experiments were begun in February, 1900, and continued until June of that year. Mr. Schiller (S.), Mr. Bair (B.) and Miss Earhart (E.) acted as subjects. All had had some previous psychological training, and were careful, interested observers.

We shall consider the results obtained from S. first. The experiments were performed by taking a series of readings under normal conditions lasting over a period of three or four minutes, and then one or two series of readings under the influence of different kinds of stimulation during the last four or five minutes. The stimuli used for S. consisted of the pain from an induction current applied by dry electrodes to the left

[1] This *Journal*, p. 313.

hand, the complex set of impressions obtained from smoking, and the odors of balsam and ether. The induction current was adjusted to give a decidedly painful sensation, but of course it was impossible to obtain any measure of its strength or even to keep it constant during the experiments. The smoking was peculiarly pleasant to S. as he had been an inveterate smoker, but had abstained for some months before the experiments. It is not meant that this gave a pure sensation of any kind or that the experiment is not complicated by the direct narcotic effect of the tobacco. As will be seen in the discussion of the results we are concerned only to show that the attention waves respond to various influences in much the same way that the better understood physiological rhythms respond, rather than to trace accurately the effect of definitely isolated stimuli upon them. Only one tracing was taken with the odors, and the odors chosen were some that happened to be on hand, and are not those likely to give the most typical results. The same objection is of course to be brought against the ether as against the nicotine, and can be met in the same way. The results obtained from odors are only of value in confirming those obtained from other substances.

The results of the experiments on S. are given in Table I. The first three columns show the time in seconds of the visible period, of the period of invisibility, and the total period under the normal conditions. The second series of three columns gives corresponding values obtained during stimulation by the induction current, and the third series, similar values while smoking. At the bottom of the Table is shown the result of the stimulation by the different odors. Each tracing is given separately with its date, that the variations from day to day may be noted. Each tracing contained from eighty to ninety complete waves, and the Table gives the results of 850 observations. Several of the early tracings were omitted that the training might be nearly constant throughout.

The most notable fact in the results of the experiments is that the length of the total wave is shortened by the electrical stimulation. It is also noticeable that the greater part of the shortening comes during the period of visibility,—the visible period is decreased in length more than a second, while the period of invisibility is only ¾ secs. less. The results of the experiments while smoking are no less marked than in the preceding case, but the changes are in the opposite direction. In these tracings the total length of the wave is very noticeably increased, but in the present instance the increase is entirely in the visible portion of the wave, while the period of invisibility is actually decreased. These statements hold not merely for the average as a whole but for the average of

TABLE I.

Duration of Attention Waves in Seconds.

Reagent S.

	NORMAL.			INDUCTION CURRENT.			WHILE SMOKING.		
TRACING DATE.	Visible	Not Visible	Total	Visible	Not Visible	Total	Visible	Not. Visible	Total
Mar. 21 (8)	6.3	5.1	11.4	5.4	5.5	10.9	—	—	—
Apr. 2 (11)	4.5	3.8	8.2	—	—	—	6.9	3.7	10.6
Apr. 2 (12)	—	—	—	4.7	3.8	8.5	6.0	3.0	9.0
Apr. 4 (15)	6.9	4.4	11.3	—	—	—	9.6	5.2	14.7
Apr. 4 (16)	6.8	4.0	10.8	5.4	3.7	9.2	—	—	—
Apr. 12 (18)	4.8	3.7	8.5	3.3	4.5	7.8	6.1	5.0	11.1
May 2 (19)	6.3	6.5	12.8	3.8	4.8	8.6	—	—	—
Average[1]	6.0	4.9	10.9	4.5 WITH ETHER	4.5	9.0	7.1 WITH BALSAM	4.2	11.3
May 2 (20)	6.7	6.7	13.4	5.0	5.4	10.4	3.3	4.1	7.44

[1]Includes normal of 20 below.

each tracing separately. The single tracing taken with odors as stimuli shows the same general effect as those taken during the stimulation with an electric current, except that the results are even more marked. In both cases the first effect of the odor was to increase the length of the stimulation, but the second effect was to shorten it very greatly.

When it comes to a consideration of the effect of the stimuli upon attention efficiency it is evident that the results given in the Table do not afford a direct means of comparison. The length of the entire wave varies, not merely the length of one part. It occurred to us, therefore, that the ratio of the period of visibility of the gray rings to the period of their invisibility would afford a direct measure of the attention efficiency, under the varying conditions. The results were tabulated in terms of this relation and the results for all subjects are shown together in Table II. The figures show the quotients obtained by dividing the period of visibility by the period of invisibility.

It will be seen in the table that the effect upon the attention efficiency is just as marked and nearly as constant as was the effect upon the length of the attention wave. In every tracing we find that the average efficiency of the attention is decreased during the electrical stimulation and is increased during the time that the subject smoked except in the case of No. 18. This tracing had been preceded by another which was discarded because the pneumograph had been drawn tight enough to be uncomfortable. As S. had been smoking during a part of that tracing the second indulgence might be expected to be

TABLE II.

Efficiency of Attention Measured in Ratio of Visible to Invisible Period.

(Subjects S., E. and B.)

S. SUBJECT.				E. SUBJECT.			B. SUBJECT.		
Tracing.	Normal.	Induction Current.	Smoking.	Tracing.	Normal.	Induction Current.	Tracing.	Normal.	Induction Current.
8	125	97	—	13	115	123	21	236	262
11	119	—	187	14	150	87	22	214	253
12	—	126	204	24	158	133	23	222	284
15	158	—	184	25	151	183			
16	170	147	—						
18	130	73	123						
19	96	81	—						
20	100	—	E[1] 92 B[2] 81						
Ave.	125	102	170	Ave.	143	134	Ave.	223	267

[1] Odor of Ether. [2] Odor of Balsam.

less pleasurable. The smoking also had immediately succeeded the electrical stimulation, and it is probable that there had not been sufficient time allowed for complete recovery from that effect. The two odors also show a reduction of attention efficiency, but again the observations are so few in number that they can be regarded only as confirmatory in character.

The results from E. show variations from those just reported, and are on the whole not so unambiguous. The results as regards the length of the waves are brought together in Table III. The effect upon attention efficiency is shown in Table II above. The Table itself is easily understood from the descriptions of the preceding Tables.

TABLE III.

Duration of Attention Waves.

E. Subject.

	NORMAL.			WITH INDUCTION CURRENT.		
No. and Date.	Visible.	Not Visible.	Total.	Visible.	Not Visible.	Total.
13 (Apr. 2)	10.1	8.8	18.9	14.6	11.9	26.4
14 (Apr. 2)	12.2	8.1	20.3	10.2	11.7	21.8
24 (May 7)	19.4	12.3	31.7	18.9	14.3	33.2
25 (May 7)	19.3	12.8	32.0	24.5	13.4	38.0
Ave.	15.3	10.5	25.8	17.1	12.8	30.0

It will be seen that here again the effect of the electrical stimulus is well marked and constant but that it is in the opposite direction to that shown by S. There is a decided lengthening of the wave rather than a shortening. There is evident in these tracings the unusual length of the attention waves that Slaughter found for the same subject. If we accept his conclusions that the attention waves of E. are related to a different physiological rhythm it would be easy to explain the apparent anomalies in the results as compared with the other persons. There are also anomalies in the effect of the stimulation upon attention efficiencies. It will be seen that in two tracings the attention efficiency is considerably increased, while in the other two tracings recorded the efficiency is very markedly decreased. These anomalies can all be discussed to better advantage after we have considered the concomitant effect of the current upon the breathing rhythm.

Table IV gives the corresponding results for B.

TABLE IV.

Duration of Attention Waves.

Reagent B.

No. and Date of Tracing.	NORMAL.			WITH INDUCTION CURRENT.			AFTER STIMULATION.		
	Visible	Not Visible	Total	Visible	Not Visible	Total	Visible	Not Visible	Total
21 (May 3)	4.9	2.1	7.0	5.2	2.0	7.2			
22 (May 4)	6.0	2.8	8.8	6.8	2.7	9.5			
23 (May 4)	6.0	2.7	8.7	7.9	2.8	10.7	5.4	2.8	8.2
Ave.	5.6	2.5	8.1	6.6	2.5	9.1	5.4	2.8	8.2

Here we see that the results of stimulation are again marked, but are in every case directly opposed to those noticed in S. In each the wave is slowed and the attention efficiency is increased. The only explanation to be offered for this result is that B. is of a very phlegmatic temperament and in perfect health.

In general, then, we find that stimulation by the induction current affected S. by uniformly quickening the rhythm of the attention fluctuation and decreasing the efficiency of the attention, for B. it just as uniformly slowed the rhythm and increased the efficiency, while for E. the wave was uniformly lengthened, but the efficiency was now increased, and now decreased. Smoking (a pleasant stimulus) lengthened the waves and increased the efficiency of the attention for S. The others were not tested for pleasurable reactions as neither smoked and no definitely pleasant stimulus suggested itself. The general results are just as confused as have been the results of re-

cent investigations upon the influence of pleasure and pain upon the volume of the members.

The results so far obtained, both from the definiteness of the fact of a reaction of some kind and the great divergence in the results for different people, suggested that it might be of value to make a simultaneous study of the effects of these stimuli upon the respiration. The respiration was chosen because of the ease and certainty with which the results could be obtained. In the light of Slaughter's results it would have been much more desirable to have made the comparison with the Traube-Hering waves, but the uncertainty of their appearance and the difficulty that attaches to obtaining a good plethysmographic tracing under varying stimulation compelled us to let that go over to another time.

During this later series of experiments the respiration was recorded on the same drum as the attention waves by means of a Fitz pneumograph and a Marey tambour that wrote parallel to the one that recorded the attention waves. The results show in most cases a correspondence between the effects upon the respiratory and the attention rhythms.

The results are collected in Tables V, VI and VII below.

There are again individual differences, so that each subject must be discussed separately. For S. we find a perfect correspondence between the breathing and the attention waves. The electric stimulation shortened both, while smoking lengthened both. The electrical stimulus had an opposed effect upon respiration and attention waves for the other two subjects. The breathing is quickened while the attention waves are slowed. This would not seem strange in the case of E., for, as we have seen, her attention rhythm is undoubtedly correlated with an entirely different physiological process from that effective for S. and B., but for B. we can only assume that a stimulus strong enough to shorten the breathing waves was not strong enough to more than slow the vaso-motor or attention waves. There is another series of complexities that arises when we consider attention efficiencies. These for S. and B. take exactly the same course as the length of the waves, but for E. we find that in one case the attention efficiency is increased while the breathing rate remains practically constant, and in another case it is decreased when the respiratory rhythm is lengthened, although in both cases the rate of the attention waves is slowed. This seems to indicate, if we use the effect upon respiration as a measure, that the first stimulus was only strong enough to facilitate the discharge of cortical cells, while in the other case it impeded the discharge of the cells or rendered them more liable to fatigue.

Taking all our results together and in connection with those

TABLE V.

Comparative Effect of Stimuli upon Attention and Respiration.

Subject S.

No.	Stimulation.	Visible Period.	Non Visible.	Total Respiration.	Attention Efficiency.
16	Normal	6.7	3.9	3.8	170
16	Induction	5.2	3.8	2.4	136
18	Normal	4.8	3.7	3.6	130
18	Induction	3.3	4.5	3.0	73
18	Smoking	6.1	5.0	6.5	123
19	Normal	6.3	6.4	5.2	98
19	Induction	3.8	4.8	3.9	81
20	Normal	6.7	6.7	6.9	100
20	Ether	5.0	5.4	3.8	92
20	Balsam	3.3	4.1	3.7	81

TABLE VI.

Effect of Stimulation upon Respiration and Attention Waves.

Subject E.

No.	Conditions of Experiment.	Visible Period.	Not Visible.	Total Respiration.	Attention Efficiency.
24	Normal	19.4	12.3	5.7	158
24	Induction	18.1	14.3	3.1	133
25	Normal	19.3	12.8	3.7	151
25	Induction	24.5	13.4	3.7	183

TABLE VII.

Respiration and Attention Waves.

Subject B.

No. of Tracing.	Conditions of Experiment.	Visible.	Not Visible.	Total Respiration.	Attention Efficiency.
21	Normal	4.9	2.1	3.2	236
21	Induction	5.2	2.0	2.8	262
22	Normal	6.0	2.8	3.5	214
22	Induction	6.8	2.7	3.1	253
23	Normal	6.0	2.8	4.3	222
23	Induction	7.9	2.8	2.7	284
23	After Stimulation Ceased.	5.4	2.8	2.9	196

obtained by Slaughter on the effect of voluntary exertion, it would seem that we may distinguish four different effects of stimulation upon attention waves. The stimulus at a certain intensity, or with some persons, shortens the length of the waves and lessens efficiency, other stimuli or the same stimuli with other subjects lengthen the waves and increase efficiency,

still other stimuli or the same stimuli on other individuals lengthen the wave and decrease the efficiency, while in still another case (voluntary effort) the waves are shortened and the attention efficiency increased. In the light of these contradictory results it would seem that we must distinguish carefully between the two different aspects of the attention and must decide that the two effects are not due to the same agencies. If we should consider a single stimulus in its varying intensities it would seem that the first influence at slight intensities is to quicken the respiration, to lengthen the vaso-motor waves and to render the attention more efficient. With a slight increase in the stimulus the vaso-motor waves are shortened, while the attention is rendered more efficient. With a yet greater increase in the intensity of the stimulus the vaso-motor waves are shortened and the attention efficiency decreased. The respiration rate is quickened by all the stimuli that we used. This series would account for all of the results except two tracings from E., and as we have noticed repeatedly these might be accounted for by the fact that her attention waves are of a different nature from those of the other subjects.

Physiologically it would be very easy to explain the different reactions of efficiency and length of wave if we think of the reinforcement from the medullary centers as merely marking off the rhythm of the fluctuations, while the variations in efficiency of the attention are explained as due to direct facilitation or inhibition of cortical activity. Then the rate would be influenced by all the factors that affect the vaso-motor rhythm just as the heart rate or respiration rate is affected, while the efficiency would need to be explained as due to a direct reinforcement or inhibition of the sensory cells as on the motor side the knee-jerk is affected by all sensory and motor impressions. It is easily conceivable that a stimulus strong enough to shorten the rhythm of the vaso-motor center would merely facilitate the action of the cortical cells, and that the relative susceptibility of these different cells would vary from time to time in the same individual and be different in different individuals.

We have not raised the question as to whether the intensity of the stimulus or its pleasantness or unpleasantness is responsible for the different changes that we notice, and our results are not sufficient to decide that question. We can at least assert that the feeling tone is only subsidiary, for we find that a decidedly unpleasant stimulus produces the same effect upon B. that a definitely pleasurable one has upon S. These and many other problems are suggested by our results, but we did not have time to go into them sufficiently to furnish a basis for discussion.

If we turn now to the direct bearing of our results upon the con-

troversy as to the nature and origin of the waves, the evidence is much clearer. The main thing in this connection is that the waves are influenced by external stimuli in very much the same way that the rhythms of heart and respiration are influenced. The direction of the influence and the nature of the stimulus are matters of very slight concern from this point of view. There is as much regularity in the effects as there is in the effect of similar stimuli upon either of the physiological rhythms or upon volume changes in the members.

But before we go on to our own explanation of the phenomena let us see if it is possible to explain the results in terms of either the Münsterberg[1] or the Leipzig theories. First as to the peripheral explanation. Is there any likelihood that a rhythm of fatigue and recovery in the muscles of accommodation in eye or ear could be influenced in one way by one kind of stimulus, in another way by another? There can of course be no direct effect upon the muscles in question, and even assuming that a muscle has a rhythm of fatigue and recovery that is of such short duration that it is very difficult to see how it could be affected by a central stimulus in the ways observed. Moreover, if there were direct stimulation we should expect it to show uniformity of results. As the action must be assumed to be of the same stimulus upon the same set of cells, the results from different individuals and at different times should be identical. Were the peripheral theory not already refuted by other facts it could offer no explanations for the phenomena that we have observed.

A very similar objection would hold against the purely central theory of Lange[2] that the fluctuations are due entirely to changes of the other ideas in consciousness at the time. This would go far towards an explanation of the oscillations in retinal rivalry, of the changing interpretation of figures in ambiguous perspective, and of the direction of the attention in general, but there is nothing at all in the ideas to account for a rhythm of any kind. If we lack a basis for an explanation of the fact of rhythm, we all the more lack any explanation of the changes in that rhythm under the influence of stimulation.

The simple Lehmann[3] theory would not be satisfactory because, in the first place, he did not hit upon the right circulatory rhythm and, in the second place, changes in blood pressure alone would not account for the variations in the efficiency, as all of the stimuli used would produce vaso-constriction while we see that they now reinforce, now weaken the attention.

[1] Beiträge zur exper. Psychologie, II, pp. 69, ff.
[2] Phil. Stud., IV, pp. 390, ff. [3] Phil. Stud., IX, pp. 66, ff.

All of our results on the other hand become intelligible if we accept Slaughter's[1] conclusions that the rhythm of the attention depends upon the re-enforcement of the cortical centers by the intermittent discharges from the medullary centers and add to his explanation the statement that the general tone of central activity is raised or lowered,—that the discharge of cortical cells is re-enforced or inhibited by the action of external stimuli. We already have an indication of the effect of irrelevant processes to re-enforce the attentive processes in the work of Professor Müusterberg[2] and Miss Hamlin[3] on distraction.

One interesting subsidiary result of the work in correlating the respiration with the attention wave was a confirmation of Lehmann's result that the changes in the attention tended to come near the beginning of inspiration. Our results are not quite so clean cut as Lehmann's, but for both S. and B. the great majority of the changes come either during or just after inspiration. For S. 83% and for B. 74% came within less than one-half of the curve following the beginning of inspiration. For E. again the results are not clear, but, nevertheless, rather more than half of the changes in attention fall within this portion of the curve. There was also noticed a tendency for the attention waves to cover even numbers of respirations over considerable intervals of time. Most frequently for S. two respiratory periods corresponded to one period of visibility and one to a period of invisibility. This changed under stimulation to one respiratory period to each period of visibility and invisibility. That there should be some such more or less definite relation follows as a corollary from the fact that changes tended to occur at definite times in the respiratory rhythm. Both relations are easily understood in the light of Slaughter's conclusions that the attention waves are due to overflows from the medullary centers to the central nervous system in general. Although the respiratory wave is not strong enough in itself to overcome the Traube-Hering vaso-motor wave, it nevertheless asserts itself at the beginning and end of the other. If the most active part of respiratory activity comes just before the longer wave is becoming sufficiently strong to bring the gray ring to consciousness, it will assert itself and make the impression visible before it otherwise would appear, and in the same way if the vaso-motor impulse is waning during the period of inspiration the respiratory re-enforcement will keep the rings in consciousness until the end of its period of activity.

These facts have some bearing upon the physiological nature of the vaso-motor rhythm. For if it is affected in one way by

[1] *Amer. Jour. of Psy.*, as quoted.
[2] Psych. Review, I, pp. 39, ff.
[3] *Amer. Jour. of Psych.*, VIII, pp. 1, ff.

the stimuli while the respiration is affected in another way it can hardly be regarded as originating in the respiratory center as Hering and Mayer have suggested.

SUMMARY.

1. The length of the attention waves is increased by stimuli of slight intensity, diminished by stimuli of greater intensity.
2. The efficiency of the attention, as shown by the ratio of the period of visibility of minimal stimuli to the period of invisibility, is increased by slight stimulation and decreased by more intense stimulation.
3. A large proportion of the changes in the attention take place during or just after inspiration.
4. The results of the experiments as a whole tend to confirm the theory that the attention waves are due to overflow effects from the vaso-motor and respiratory centers upon the cortical centers.

Our results suggest that the Traube-Hering and other circulatory rhythms can be more conveniently studied in man in their secondary form as attention waves than directly by the plethysmograph.

III.

DOES THE SENSATION OF MOVEMENT ORIGINATE IN THE JOINT?

By W. B. Pillsbury.

Since Goldscheider's work on the 'Muscle Sense' there seems to be very general agreement that the joint is the only or by far the most important source of the sensations that inform us that we have moved the members of the body. Almost no question has been raised during the decade that has elapsed as to the absolute completeness and exactness of his results. During the last few years, however, the conviction has been growing upon the writer that some of this work needs revision or extension, and that the dominant part which is ascribed to the joint in the complex is not definitely proved to be deserved.

Goldscheider's analysis of the sensations received during passive movement makes it consist (1) of the sensations that arise from the rubbing of articular surfaces, together with wrinkling of the capsule; (2) strain upon the tendons of one set of muscles and relaxation of the tendons of the antagonists, and (3) change in the form of the muscles. A striking divergence in the facts of consciousness from what this analysis suggests is seen in the majority of subjects who work with the sensation for the first time in that the movement seems to be noticed first in the wrist or fore-arm, or even in the tips of the fingers. A possible explanation of this fact in terms of Goldscheider's theory might be offered if we considered the sensation in the wrist merely the result of an associative projection of the joint sensation to the part that is most concerned in the movement. This would bring it under the same category as the projection of sensations to the tip of the pen in writing, or to the end of the cane in walking. This was the explanation that was offered to the students, and that for some time seemed entirely satisfactory. On one occasion, however, it was suggested to two students who were suspicious about this explanation that they pass an induction current through the wrist and see if it had any effect. It was supposed that there would be no effect of any kind, but on the contrary it was found that there followed nearly as marked a decrease in sensibility as when the current was passed through the elbow itself. This observation was confirmed on several subjects and with several

successive classes until there seemed to be no longer any doubt that it would be profitable to work over the field again in spite of the general acceptance that is accorded to Goldscheider's conclusions.

Experiments were begun in November, 1900, and continued until the following January. Mr. Bair (B.), Mr. Stevens (S.) and the writer (P.) acted as subjects. All had had some previous psychological training. Experiments were made upon the elbow and knee joints only. The apparatus used was the ordinary passive movement apparatus, a hinged board to support the arm which was raised by a cord that passed over a pulley fixed in the top of an upright. To avoid the unevenness in speed that is necessarily connected with moving the board by hand we arranged to lift it by an electric motor. The speed of the motor was reduced by a worm gear and a series of pulleys. An ordinary clutch in an old bit of shafting served to interrupt the movement of the board without stopping the motor. The connection between shafting and arm board was made by an elastic cord. This served admirably to reduce the jerk at starting. The slack was taken up gradually, and the movement began almost imperceptibly. When once started the movement was at a constant rate. A pointer in the end of the arm board moved over a millimeter scale on the upright that supported the pulley and served to measure the amount of the movement of the arm.

In the first series of experiments the subject sat with the arm upon the board, with the elbow joint just over the hinge, the clutch was adjusted to start the board, and a signal was given as the arm began to rise. This time for giving the signal was adopted after several trials because it was the only one that permitted a constant interval to elapse between the time of giving the signal and the entrance of the sensation into consciousness. It could not be given when the clutch was adjusted to start the movement because it was impossible to keep the amount of slack to be taken up a constant. The movement was so slow (about 20′ per second) that it ordinarily took about two seconds for the movement to progress far enough to be noticed. The first series of experiments were taken with the arm normal, then a series was taken with the induction current passing through the elbow, a third with the current passing through the wrist, and finally several series with both elbow and wrist anæsthetic. In each case the current was passed as nearly as possible directly through the joint to be experimented upon. The results are collected in Table I.

In the Table the first pair of columns gives the results with the normal arm, the second those with the current through the elbow, the third with the current through the wrist, and the

TABLE I.

SUBJECT.	NORMAL.		CURRENT TO ELBOW.		CURRENT TO WRIST.		WRIST AND ELBOW.	
	No.	M.	No.	M.	No.	M.	No.	M.
B.	118	33′	81	1° 45′	70	1° 28′	85	2° 8′
S.	163	51′	76	2° 30′	68	1° 43′	76	6° 27′
P.	179	26′	68	1° 50′	68	2° 2′	70	2° 24′

last with the current through both wrist and elbow. The first column in each pair gives the number of experiments, the second, the average just noticeable movement. It will be seen that in every case there is a very noticeable diminution in the sensitivity to movement at the elbow when the current is passed through the wrist. In one case the minimal movement is considerably less than when the elbow joint is anæsthetised; in a second case is slightly less, and in a third case is slightly greater, but in no case is the average movement necessary to produce a sensation less than twice as great as that required in the normal arm. A confirmation of the result is found in the fourth series of experiments in the fact that when the current was passed through both joints at once there was a marked decrease of sensitivity as compared with either of the other conditions. Any suspicion that these results may be due to suggestion or may be artefacts of the method of procedure is removed by the facts that the sensitivity decreased constantly as the series of experiments proceeded, would increase again if for any reason the experiment was resumed after temporary interruption with the current cut off, and that the anæsthesia persisted with diminishing force for some time after the current had stopped.

The same series of experiments was repeated with the knee joint. In these experiments a very similar apparatus was used as for the elbow. The board used was long enough to accommodate the lower leg, the subject lay face down upon a table with the knee-joint just over the hinge. The lower leg was supported by the knee and the toes so that no other parts were in contact with the apparatus. The position was uncomfortable, and it was, therefore, necessary to give the subjects frequent rests to prevent excessive fatigue. The discomfort, however, would affect all results in the same degree, and so would not detract from the relative correctness of the results. The differences are also too large and too constant to be accounted for by any error of this kind.

The experiments were repeated for the knee in very much the same order and with the same results as in the preceding case. The results are shown in Table II below.

TABLE II.

Movement of Knee Joint.

SUBJECT.	NORMAL.		TOE ON SPONGE.		CURRENT THROUGH TOE		KNEE.		ANKLE.		KNEE AND ANKLE.	
	N.	M.	N.	M.	N.	M.	N.	M.	N.	M.	N.	M.
B.	239	1° 15′	—	——	—	——	85	2° 46′	97	2° 8′	86	3° 12′
S.	128	1° 27′	59	1° 18′	133	1° 18′	126	2° 46′	115	2° 46′	—	——
P.					123	41′	150	2° 22′	105	2° 11′		

Here, too, anæsthesia of the joint distal to the one moved had a marked effect in decreasing the sensitivity of the latter. The results are again usually not so great as in passing the current through the moved joint, but are not so very different from it. In the one case in which the two joints were anæsthetised at the same time there is again a marked increase over the effect upon either alone. For P. and S. this series was not taken as the other two were in complete agreement, and did not seem to need confirmation.

Incidentally in the series with S. we obtained an interesting comparison of the sensitivity of the skin and the deeper lying sense organs. S. noticed during the first series of normal experiments that there was a sensation coming from the skin of his toes, and feared that he might be making his judgment in terms of the tactual rather than of the kinæsthetic sensations. When the experimenter watched carefully it was seen that as the member was raised the toes were forced upward along the board rubbing the skin as they moved. This movement at times amounted to as much as two millimeters. To guard against this source of error the toe was first placed upon a dry sponge which moved along the board and saved the toes, which it supported, from the rubbing. It will be seen in the second column of Table II that the only result of this arrangement was actually to decrease the least noticeable movement. Fearing that there might still be some sensation from the skin that enabled the judgment to be made, the sponge was moistened and connected with one pole of the induction coil, another electrode was placed on the other side of the toes, and the current passed through. It will be seen from the table again that the result under these conditions gave exactly the same average as in the preceding case. The sensation from the skin, then, has apparently absolutely no influence upon the judgment. Under these conditions the limen for the tactual is either higher than or identical with the limen for the kinæsthetic sensations. The subject's impression that the judgment was in terms of the skin must have been due to the fact that the board was not

stopped quickly enough after he had made his judgment to prevent overstepping the limen for the tactual sensations as well as for the kinæsthetic, and the distinction between the sensations that came before and those that came after the judgment could not be clearly drawn. These experiments with current through the toe would also remove any possibility of the effect of the induction current being due to the distracting effect of the pain. Here we find the same strength of current actually resulting in an increased rather than a decreased sensibility, if we are to draw any conclusion at all from differences so slight.

The experiments upon B. had already been made before these observations, and in the light of them it did not seem necessary to discard the earlier ones. The experiments on P. were all made, assuming the results obtained with the current through the toe as the standard of reference.

Our results then go to show very definitely that the sensibility to movement in elbow or knee is decreased nearly as much by passing a current through the ankle or wrist as by passing it through the joint that is concerned. Our next question is as to how this fact can be explained in the light of what we know of the sensory nerve supply of the different parts, and how they can be brought into harmony with the results of Goldscheider in the same field.

It seems evident at once that the conclusions of Goldscheider that sensation of movement is mainly due to the excitation of the sensory endings in the joints by the rubbing of joint surfaces cannot be accepted without further investigation and modification. Certainly the nerves at the elbow are not likely to be rendered less sensitive by the passage of the current through the wrist, and on his premises there seems no possibility of any other explanation.

Let us turn to the anatomical facts in relation to the distribution of sensory nerves and see what light these cast upon the subject. The undisputed facts as to endings that may be concerned is first that there are highly developed sensory endings thickly scattered in the tendons, particularly in the zone of transition from muscle to tendon (Huber and DeWitt,[1] Golgi,[2] and others), that there are sensory endings in the tissue of the muscle, and that Pacini corpuscles are found embedded in adipose tissue between the tendons and muscles, and are particularly frequent in the neighborhood of the joints (Rauber).[3]

[1] A Contribution on Nerve Terminations in Neuro-tendinous Organs: Jour. of Comp. Neurology, Vol. XII, pp. 159, ff.

[2] *Sui nervi nei tendini dell 'uomo*, etc. Memor. della R. Accad. delle Sc. di Torino; Serie II, Tomo XIII, 1880.

[3] Untersuchungen Ueber das Vorkommen u. Bedeutung d. Vaterschen Körper, München, 1865.

The sensory innervation of the joint is not so definitely made out nor so free from disputed points. The statement so frequently made in the text-books of physiology and psychology that the joints or joint surfaces are richly supplied with sensory endings seems to have grown out of Rauber's discovery that there were Pacini corpuscles in the neighborhood of the joints. This statement was, apparently, at first taken to mean indefinitely inside or outside of the joint capsule, then within the capsule, and, finally, on the joint surfaces. At least we could nowhere find any definite statement that sensory nerves were found on those surfaces, and where any authority was cited for the nerve supply of the joints the reference was to Rauber. Goldscheider gives no authority for his statement, and says definitely: 'Eine anatomische Untersuchung der Innervation der Gelenkenden wäre wünschenswerth.' Ivanhow[1] found sensory endings in the fasciæ and the capsules of the joints, but this seems to be the only well established histological evidence of sensory endings in the joints themselves.

In the same article Goldscheider[2] attempted to prove physiologically that the joint surfaces were sensitive, but with poor success. He exposed the joint surfaces in frogs and rabbits, and stimulated them by pressure and heat, in the hope that he might excite respiratory reflexes. His results were of a negative kind. He did call out reflexes when the bone was cut away to expose the marrow, but with the uninjured joint surfaces there was no definite response, that could not be interpreted as due to the transmission of the stimulation to the periost or to the marrow. He concludes, "Dass die Gelenkfläche selbst empfindlich sei, hat durch die Versuche nicht erwiesen können," but adds, "Aber auch ohne dieselbe darf wohl die Berichtigung, die Gelenkenden als Substrat einer Sensation anzusehen, bereits anerkannt werden." The grounds upon which he rests this conclusion in the earlier article are: first, the fact that the sensitivity is reduced by passing an electric current through the joints, then that all the other possible sense organs may be eliminated,—the skin because a superficial anæsthesia does not diminish the sensitivity to movement; the muscle and tendon because there is practically the same limen for all positions of the limb while the variations in the condition of the muscle must take place more rapidly in one position than in another. The first argument we are prepared to deal with later; that the skin does not serve as the organ, may pass without comment, while the argument that it

[1] On Nerve Endings in the Connective Tissue Capsules and fasciæ of mamalia (Russian). Dissertation, Kasan, 1893.

[2] Ueber die Empfindlichkeit d. Gelenkenden: Gesamm. Abhandlungen II. pp. 282, ff.

is neither muscle nor tendon, seems to have taken into consideration only the flexors, and to have forgotten the extensors, which would be affected in the opposite way in all respects, and serve to counteract any irregularities to be expected from the flexors alone, and also to have overlooked the muscles that were attached about the joint which do not serve to bend it. The evidence in favor of the joint surfaces as the seat of the sensation then is reduced to the fact that the sensitivity of the joint is reduced when the current is passed through it.

Our results cannot easily be explained in terms of Goldscheider's hypotheses, but can easily be brought into line if we assume that the sensation of movement originates in the tendon organs or in the Pacinian corpuscles which Rauber described. This becomes clearer from a glance at the arrangement of muscles and tendons that run from elbow to wrist, and from knee to ankle. Many of the large muscles, both flexor and extensor, have their origin above the elbow on the lower end of the humerus and their insertions at the lower end of the radius or ulna, in the metacarpi or even in the phalanges of the fingers. The upper tendons of all these muscles cross the elbow joint, and the lower tendons are thickly gathered together at the wrist. Any movement of the arm must tend to relax the tension on one set of muscles and tendons, and to increase that on the other set. Very similar relations hold for the lower leg. The sensations that are produced by these changes in tension would come from about the elbow and knee in part, but also in part from the wrist and lower forearm, and from ankle and lower leg. Anæsthesia of either set of tendons would then be expected to produce a decreased sensitiveness to movement, as we found in our experiments. Furthermore, we should expect that the current through the elbow and knee would have the more marked effect, as the tendons at the lower ends of the muscles begin to appear well up on the forearm and lower leg, and so a smaller portion of their length would be affected by the current, and, secondly, the tendons at the insertions of the muscles of the upper arm and thigh, biceps and triceps, *e. g.*, would be affected at the elbow and knee, and not at the wrist or ankle. Another set of organs to be affected are the Pacinian corpuscles described by Rauber. These are most numerous about the joints, and any change in the tension upon the muscles and tendons would also change the pressure upon them. Altogether, then, our results would be satisfactorily and completely explained on the assumption that it is the tendon and muscle organs, not the joint surfaces, that are the source of the sensations of movement.

If it is still insisted that the joints are sensitive and originate the sensation, it is only possible to explain the reduction of

sensitivity by the current through the wrist and ankle on the assumption that the muscles and tendons acted as cords, and that the displacement at the elbow or knee produced an increase or decrease in the pressure of joint surface upon joint surface at the wrist and ankle. Even if the joint were extremely sensitive it is hardly conceivable that the tendons could transmit sufficient energy to produce a noticeable effect there without exciting the delicate sense organs in their tissue. Particularly would this be the case if we consider the probability that there would be only a change in the disposition of the pressure upon the surfaces rather than a change in the amount of the pressure.

Looked at from every side, then, it seems very difficult to avoid the conclusion that the part of the sensation of movement which originates in the wrist is due to stimulation of the tendon organ. If we are forced to this conclusion in the one case, it certainly seems easier in the light of the doubt that surrounds the innervation of the joint to assume that the sensation from the elbow is also a tendinous or muscular sensation rather than one from the joint. The forces that stimulate the lower tendon are at work in even greater degree in the upper, and they are alone sufficient to account for the known effects. Any bringing in of joint sensation would at the least be entirely gratuitous. What part the sensory endings in fasciæ and capsule may play does not appear. However, from the fact that they are relatively less developed than the tendon and muscle organs, and that they are probably less favorably situated for stimulation, it would seem probable that they have a minor rôle.

Summary.

We find that the sensitivity of the joints is decreased by induction currents through wrist and elbow as well as by currents through the joints in question. This fact, together with the lack of anatomical evidence that the joints have sensory endings, makes it probable that the sensation of movement is derived mainly from the tendon and muscle rather than, as Goldscheider thought, from the joint.

THE EDUCABILITY OF THE PERCH.

By Norman Triplett.

The material here presented is a brief summary of observations made in the spring of 1899 on the psychic life of fishes in captivity. Its chief interest lies in its being a modified repetition of the famous experiment of Möbius which has come to be regarded by some as one of the fairy tales of science. Bateson's account of the experiment is as follows:[1]

"The story runs that pike, having lived for some time in a tank separated by a glass plate from another in which small fish were living, desisted from trying to catch them, and on the glass plate being removed never attempted to do so. The suggestion is that the pike had come to believe these particular fish to be under special protection."

In the experiment to be detailed herein, two perch (*Perca Americana*), one of each sex, took the place of the pike. These fish had been kept in the laboratory in a tank, 4x2x1½ feet in size, for several months previous to the test given below. During all this time their only food had consisted of live minnows two or three inches in length. On beginning the experiment a glass partition was placed in the tank and their food changed to angleworms.

Instead of permitting the minnows to remain in the divided tank as Möbius did, I simply put them in for thirty minutes, and at the end of that time removed them again and gave the perch an allowance of worms. Experiments were made in this manner three times a week for nearly a month, on Mondays, Wednesdays and Fridays between the hours of four and five o'clock in the afternoon, and after that on every day at the same hour. In all experiments after the very first the observations were made from behind a screen through a narrow slit.

The notes taken on the various occasions would tell the story most completely, but to avoid unnecessary repetition, they have been condensed in the description following. On April

[1] W. Bateson: The Sense-Organs and Perceptions of Fishes, *Journal of the Marine Biological Association*, N. S. 1890, I, 225-256; see especially p. 243. The original account by Möbius in the *Zeitschr. Gesamml. Naturwiss.*, XLII, 1873, pp. 89-91, has, I regret to say, not been accessible to me. The same experiment is cited by Darwin in the Descent of Man, pp. 75-76, with a reference *Die Bewegungen der Thiere, etc.*, 1873, p. 11.

21st, the first day, I note: Two minnows were placed in the tank at 4.30 P. M. The perch immediately began ramming the glass to get at them. Their actions became more violent as the minnows approached the partition. They ceased their butting and swam away from the glass for a few seconds after seven minutes of continuous effort. A second trial lasting one minute followed, and this was followed in turn by still shorter periods. Greater energy was shown always when the minnows turned their heads toward the perch, as it is only when they are "head on" that the latter strike. At 4.55 the female was showing what seemed to be signs of anger, and was striking the glass hard. During all the subsequent trials she was the more persistent of the two, and several times seemed to show signs of rage, lashing the glass partition savagely. By five o'clock both perch had left the glass and seemed to have given up the attempt completely. At this stage the minnows were removed and the perch fed.

At the next trial their efforts were not so long continued nor so violent as on the first day, and the further history of this part of the experiment varies but little, the time and energy spent at the glass fluctuating somewhat from day to day, depending perhaps in some degree on the keenness of their hunger. Their efforts on Mondays were noticeably more determined than on Wednesdays and Fridays, because, it would seem, of the added day's fast. They never failed to try for the minnows to some extent, especially during the first half of the thirty minute period, but with waning energy and persistence. In this manner the trials proceeded for a month. On May 22nd I note: They pay no attention to the minnow till some time after I have placed myself behind the screen. They are less demonstrative than on any previous Monday.

I now determined to admit the minnow, and after the appetite of the perch had been dulled a very little with worms, the partition was removed. The minnow swam around with the perch, over and between them. Having lost its own mate, it perhaps sought companionship. The male paid not the slightest attention to it. The female, whose persistence at the glass has been noted, moved toward it several times, but whether from curiosity or with hostile intent could not be determined. She did it no harm however. On succeeding days the minnow was admitted in the same way, and the actions of the perch toward it were closely observed. On the second occasion they had been fed little and were still hungry. The minnow hovered near them as if for companionship, but all the time was a little wary. Several times they started to stalk it, but when within a short distance of its head would turn aside, the impulse being plainly inhibited in the face of repeated opportunity. In the

subsequent trials the perch were unfed. Their action, nevertheless, was always much as has been indicated. Sometimes they would swim around the aquarium apparently quite indifferent to the minnow, which accompanied them, but the sight of it moving in front of them would often stimulate the truncated attack already described.

The form of the experiment was now somewhat changed, being performed as nearly as possible after the manner of Möbius. The minnows, the one used before and a new one, were left for a week in the tank, separated from the perch by the glass. The latter ceased almost entirely to touch the glass, although quite frequently they took a position near it and watched the minnows playing on the other side within two or three inches of them. Their conduct when the partition was removed was exactly similar to that in the previous experiments.

Bateson, in the article above mentioned, suggests that the result of the experiment of Möbius may have been wrongly interpreted, and that "the explanation should be referred to that paradoxical instinct which is widely developed among animals of many kinds, in obedience to which they occasionally do not eat or molest those with whom they are constantly associated. It is, of course, this unexplained instinct upon which the 'happy family' of the travelling showman is constructed." In the case of these perch, however, the "happy family" theory will hardly serve. The perch's whole attitude is expressive of a desire to catch the minnow, a task which has come, however, to be regarded as hopeless. Such an explanation is also made less plausible by the fact that different minnows of different size were tried in the course of the experiment. And, indeed, habituation by constant association is hardly to be considered here; for the minnow, at least in the first series of experiments, was exposed to the sight of the perch but a short time each day, and at the period of greatest hunger, which furnishes in itself the strongest possible stimulus, not to the production of feelings of friendliness, but to the calling forth of savage instincts. That they were acting under the suggestion of their previous failures seems, in the light of the facts observed, the most reasonable explanation. The following incident is confirmatory of this. Toward the close of the work, when the perch had ceased to bump the glass in their efforts to reach the minnows, some angleworms were thrown into the tank on the side of the partition opposite the perch and lay squirming on the floor. The fish dashed violently against the partition in their effort to reach them, and beat the glass energetically for some time.

Their behavior in the presence of a strong suggestion, counter to their newly acquired habit, also militates against the view offered by Bateson. About the end of the third week,

while attempting to remove one of the minnows from the aquarium, it slipped through a crevice at one end of the partition into the part occupied by the perch, and dashing directly toward them in its flight was snapped up like a flash. The rapidity of its movement was probably the prime incentive, and called out the normal tendency to strike, before the later acquired inhibition could come into action. This minnow, or its mate, had gained entrance to the perch in the same way a few days previously, but in this case in a less exciting manner. The perch, aside from the female's nudging its tail twice, paid no attention to it, although it swam around them and under their very noses.

The restraining influence under which they were acting was again broken a few days later. Circumstances made necessary the omission of the test till two days after the regular period had passed. They were therefore wild with hunger and the opportunity for observing a conflict of impulses was of the best. The minnow was admitted, and at first was treated much as on other occasions. After a few moments, however, being evidently impelled by hunger, the perch became more demonstrative toward it, but restrained themselves from striking, so long as it quietly avoided them. Finally, however, being followed into a corner, it made a quick dart to escape, and this was apparently all that was required to wake the sleeping instinct, and a savage chase began which must have ended fatally to the minnow, had I not separated them. The perch were thoroughly aroused and all effect of their training seemed, for the time being, to be lost. A few more bumps at the glass during succeeding trials, however, restored the inhibition, and the final test made a little later showed it to be quite firmly impressed upon them. The minnows were removed from the sight of the perch for five days. To ascertain whether any effect of the training yet remained, they were then put directly into the compartment with the perch which had eaten nothing for two days. It was plain to the observers that the behavior of the latter had been greatly modified by the course of education to which they had been subjected. The first minnow put in soon attracted their attention. Their interest increased as they followed it about, and the female began to strike at it with growing boldness, and getting it into a corner finally secured it. The second minnow was greatly desired by the male, but he seemed utterly inefficient. He would trail it around and at times make feeble darts at it and then give up the attempt for the moment. In this way he was seen to nag it for nearly an hour. On returning to the tank after five hours the minnow was found swimming near the perch entirely unmolested. The male's experience had apparently so far inhibited his natural

reaction that he had ceased to try. As Paulsen remarks of the pike in Möbius's experiment, "he had manifestly made a law of nature for himself." A strike at the minnow had come to mean a bump on the nose for him.

In regard also to the general educability of fishes my observations are at variance with those of Bateson. He says with regard to the persistence with which the fish in the tanks of the Marine Biological Associon beat the glass, and their slowness to profit by experience or to form associations: "None of the fish seem to get any lasting appreciation of the nature of the plate glass wall of the tank. The same fish will again and again knock the head against the glass in trying to seize objects moving on the other side. After repeated attempts to take food on the other side of the glass they will desist, but some of the oldest inhabitants (plaice, pollock and bream) which have been living in the aquarium for about a year, will perseveringly try again the next time." This was not the case with the perch under discussion. While these fish did not entirely cease striking the glass during the time when the trials were but thirty minutes long, their attempts grew very much more infrequent and their blows feebler. Later in the changed form of the experiment, becoming accustomed to the sight of the minnows, they gave up striking the glass, merely continuing to watch them. This, in connection with their conduct toward the minnows when the glass was removed, suggests that they have at least a strong temporary appreciation of the obstacle.

Another proof that they had formed a firm association with regard to it may be taken from the notes. On April 21st the partition which had been in position for several days was removed and the perch driven toward the place previously occupied by it. On reaching it they stopped and turned back. "On May 4th, glass removed in order to clean tank, but waited to see if fish would cross the line. The male swam out to the place, stopped, made little bumps forward as if expecting to strike the usual obstruction, and was plainly at a loss. He then turned and swam down as if following the glass." Ten days later with the same conditions they swam out to the mark several times, then turned and swam back. So on a later occasion when the glass was taken out they turned three times at the mark, but finally crossed in a hesitating manner.

During the period in which they had lived on minnows the perch remained indifferent to man, but when their diet was changed to angleworms they began to take such a deep interest in the experimenter that it was necessary to use a screen in the trials. This added considerable interest to the study, especially as regards their memory and time sense. After releasing the minnows the experimenter took his place behind the

screen, viewing the movements of the fish through a narrow slit, and they saw nothing of him for thirty minutes. During this period, however, he was not forgotten. The first half of the time was pretty fairly divided between the minnow and the screen. As shown by my notes a marked change took place in the last half of the half hour period. They spent a larger and larger portion of the time gazing out toward the screen. Circling the tank rapidly a few times with growing excitement they would return to the spot, snapping their jaws and flirting their tails, till toward the close of the period they had attention for little else than the appearance of the man behind the screen. The time sense here exhibited is probably in large part a feeding-time sense; they remembered with their stomachs. The rhythmic recurrence of internal sensations due to a regular feeding time may well account for the form of memory here involved.

The same hunger memory might possibly help to explain the stories of fish coming regularly at the sound of a bell to be fed. Bateson found, in the study before referred to, that most fish hear little or nothing of sounds in the air, but are affected by vibrations of the water and earth near it. The subject has also been carefully studied by Kreidl[1] and by Lee,[2] whose experiments are in agreement with his. Lee thus summarizes the case as presented in Kreidl's second paper: "Kreidl explodes the oft repeated tale of hearing by fishes that come for their food at the sound of a bell, by investigating carefully the action of trout at the famous old Benedictine monastery in Krems, Austria. He proved that the fishes come because they see the man who brings the food, and appreciate the vibrations of the water caused by his step and communicated through the stone basin; and that, when these are excluded, the sounds of the bell have no effect." The weight of recent authorities is thus against the existence in fishes of an auditory sense in the usual meaning of the term. My own observations, though not of a character to support an opposite conclusion, seem to me to require for their explanation the existence of a "sense of jar" of a delicacy at least equal to that of hearing. I made it a practice to whistle loudly while feeding the fish, and often while behind the screen I would use the whistle in the same manner. Very often at such times they would come directly to the side and look eagerly at the screen. Often they would turn only slightly in my direction; and in a very few cases they paid little

[1] Kreidl: Ueber die Perception der Schallwellen bei den Fischen, *Pflüger's Archiv für Physiologie*, Bd. LXI, p. 450, also Bb. LXIII, p. 581.

[2] Lee: The Functions of the Ear and the Lateral Line in Fishes, *American Journal of Physiology*, I, 1898, 128-144.

or no attention. Some gold fish in the laboratory, however, hardly ever failed to come at the sound of the whistle, especially if hungry. In regard to the fish feeding stories, it must be agreed, however, that the sight of the man ringing the bell is the chief factor.

Belonging to the group of fishes that find their food by sight, the perch possess great keenness of vision. They are so highly reflex, also, that the least visual perception of motion puts them into action. Two bright buttons kept in oscillation by the incoming water were a constant source of attraction to them. The swaying objects when caught in indirect vision seemed never to fail of bringing them to attention. Of course the trolling hook, with its revolving spoon of bright metal, depends for its efficiency on this deep seated interest in bright and moving objects.

These fish are very imitative, a movement in one almost surely producing a similar movement in the other. This trait is probably due to their primitive shoal life, where it was necessary to follow the crowd or fall a victim to the host of enemies on the outskirts.

To test their power of discrimination a species of wireworm was dropped into the tank alternately with small angleworms broken to the same length. The difference to sight was very small indeed. On one occasion when five were thus given, the first three were taken into the mouth and then rejected. The remaining two were permitted to sink to the bottom untouched. In subsequent trials the first wireworm has generally been taken into the mouth and the others neglected, the discrimination perhaps being aided by taste or possibly by the sense of touch in the mouth. The fish seemed, however, to make no fully permanent associations, but always to test the possibilities of such an object falling through the water.

STUDIES OF RHYTHM AND METER.

By Norman Triplett and Edmund C. Sanford.
(*From the Psychological Laboratory, Clark University.*)

I—On the stanza forms of nursery rhymes.
II—An experimental study of the rhythms of nursery rhymes.
III—College yells.
IV—A brief collection of common rhythms with words that have been fitted to them.

I. On the Stanza Forms of Nursery Rhymes.

In attempting to recall a forgotten phrase or stanza it is by no means uncommon to recapture the "sound" or "lilt" of it, while the words escape; we recall the form or plan of the phrase, but not the filling in. At other times, as in listening to a conversation indistinctly heard, *e. g.*, by telephone, we are able, from the familiar cadence of the sounds, few of which are distinct enough to be fully recognized, to reconstruct the phrases which we can hardly be said to hear. To such groups of related sounds and others similar, considered merely as sounds, we may give the name of auditory forms.

Auditory forms bear the same relation to the things of the auditory world—phrases of speech, city noises, cries of animals and the like—as the figures of bodies bear to the things of the visual and tactual worlds. An ivy leaf or the new moon is not more characteristic and easily recognizable in its visual form than the crowing of a cock or the hoof-beat of a galloping horse in its auditory form. Visual form means certain fixed associations of visual, tactual and kinæsthetic sensations; auditory form certain fixed associations of auditory and kinæsthetic sensations. In both cases the form pure and simple is the result of a certain degree of abstraction (not necessarily conscious); the color and material of a pebble are neglected when its roundness is considered, and in the same way the words and meaning of a phrase are neglected when its rhythm is regarded. Furthermore, as there are in the visual world objects of the most varied form, some irregular, like clods and rock masses, others with regularity of plan, like flowers and fruits, and finally, objects of human construction showing the regular geometrical figures, so there are in the auditory world forms of considerable irregularity, like those of prose speech, others of approximate regularity, like lines of poetry

and phrases of music, and still others of complete regularity, like the ticking of a clock or the artificially grouped sounds of the laboratory experiments on rhythm,—these last fairly comparable to the geometrical figures—the regular chronometrical figures as they might be called.

It is the purpose of the following studies to present certain observations and experiments with reference to auditory forms of the more regular sorts, and of this section and the next to consider the stanza forms or "metrical patterns" of nursery rhymes.

These rhymes offer a natural approach to the study of poetical rhythm and meter in general. They are in the highest degree rhythmical, almost everything else being sacrificed to that, and they are simple, as they must be to suit the tastes and comprehension of little children. They owe their origin, of course, to the natural responsiveness of children to rhythmic sounds and movements. Some are clearly motion songs. The regular movements used for lulling the child to sleep or those of marching have furnished a framework which has later been given a verbal covering, and rhythmic movements are still a frequent accompaniment of some of them. Others seem to have started in the mere repetition of a single word, as "Peter, Peter," or a common phrase as "There was an old woman," or "There was a little man." The rhythm thus set was carried on as words offered and fancy suggested till a full stanza was formed, crude at first perhaps, but improving with time and as the rhyme passed from mouth to mouth. Still others have had a definite history and have entered nursery literature from popular verse at various times. Many have the irregularity of all natural and spontaneous products, and take liberties in the matter of accent and pronunciation that would hardly be tolerated in more formal verse, yet the difficulties thus introduced are not very serious. For present purposes they may be entirely neglected, for in the more familiar rhymes, at least, they affect the foot structure rather than the stanza forms.

The rhymes that have formed the basis of this study are the one hundred generally familiar ones collected by one of us in answer to a request addressed to a large number of persons for a list of ten rhymes each—if possible the first ten coming to mind.[1]

The most frequent stanza form in this list is that of four lines of four stresses each, for example,

> Georgie, porgie, pudding and pie;
> Kissed the girls and made them cry.
> When the boys came out to play,
> Georgie porgie ran away.

[1] Triplett: A Census of Nursery Rhymes, Journal of Pedagogy, April, 1901.

In nearly all cases the lines rhyme in couplets as in this example, showing the close relation of this stanza pattern to the simpler pattern of the couplet. The couplet is itself not uncommon, either standing alone or repeated as the stanza unit of longer rhymes, as for example,

> Needles and pins, needles and pins!
> When a man marries, his trouble begins.

and

> " Where are you going, my pretty maid ?
> " I'm going a milking, sir," she said.
>
> " May I go with you, my pretty maid ?"
> " Yes, if you please, kind sir," she said, etc., etc.

The 4-stress line, especially in couplets, is very ancient in English poetry,[1] and is based on what appears to be a very natural grouping for sounds of moderate frequency.[2] The simplest possible rhythmic structure is undoubtedly the grouping of stresses by twos, and of 2-stress lines there are a few examples among the nursery rhymes,—for example,

> One, two ;
> Buckle my shoe.
> etc., etc.

The next stage, antedating (one may conjecture) any attempt at a 3-stress line, is the line of two twos, or the 4-stress line. These united in the simplest possible way gives the couplet, the fundamental pattern of this group, and a pair of couplets the most frequent stanza of the nursery rhymes. The stanza of four 4-stress lines, rhyming in couplets or otherwise is also a very common one in lyric poetry generally. In hymnology it occurs as the long meter stanza (iambic) and as the four line stanza of 7's (trochaic), both extremely common hymn stanzas.

Another numerous group is that in which the stanza consists of two 4-stress lines (the first and third) and two 3-stress lines (the second and fourth), or, as it might as properly be written in most cases, of a couplet of 7-stress lines or septinaries.[3] Of these there are two varieties, one in which the 4-stress lines are broken by internal rhyme, and one in which they are not. As an example of the first we may cite

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[1] *Cf.* Schipper: Grundriss der englischen Metrik, Leipzig, 1895, pp. 108, 176.

[2] *Cf.* Bolton: Rhythm, *Amer. Jour. of Psy.*, VI, 1893-95, pp. 212, 216.

[3] No spatial arrangement of the parts of a stanza can give a parallel of the temporal arrangement presented to the ear, unless it be the placing of all the parts in a single line, with spacing to indicate the separation of the parts.

Mistress Mary, quite contrary,
How does your garden grow?
Silver bells and cockle shells
And pretty maids all in a row.

Of the second,

There was a man in our town,
And he was wondrous wise;
He jumped into a bramble bush
And scratched out both his eyes.

The first is extremely common as a nursery rhyme pattern, but rather rare in adult verse; the second very common in adult verse, but less so among the nursery rhymes. The latter is the stanza of many ballads and hymns (ballad meter, common meter) and very frequent in other kinds of verse. According to Schipper (*op. cit.*, pp. 186, 298), both are, like the 4-stress couplets, very old, having prototypes in late Latin and old French. The internal rhymes of the first variety merely carry further a frequent tendency of 4-stress lines to fall apart into two twos, but the short lines make the rhythmic effect of the stanza quite different from that of the second variety, so different in fact as to justify a separate classification. The stanza pattern would indeed be more fairly represented by printing the rhymed sections of the first variety as separate lines, thus:

Mistress Mary,
Quite contrary,
How does your garden grow?
Silver bells,
And cockle shells,
And pretty maids all in a row.

If one should attempt to describe the difference in words he might say that the form with internal rhyme gives the effect of two partial, or not wholly successful, efforts, followed by complete success on the third trial, or of two small waves followed by a third large one that runs far up the beach. In the stanza without the internal rhyme the effort is sustained throughout the whole of the 4-stress line and falls in the 3-stress line. The quick recurrence of rhymes in the first variety not only gives the stanza a quick movement and much jingle, but rhymes in such close proximity are themselves very emphatic,[1] and mark the stanza pattern in a striking way, features which would recommend it to the poet of the nursery more than to the poet of adults.

Between these two varieties stand several nursery rhymes which have internal rhymes in the first or third line (most frequently in the first), but not in both. For example,

[1] *Cf.* Corson, Primer of English Verse, p. 23, f.

See, saw, Margery Daw,
Johnny shall have a new master.
He shall have but a penny a day,
Because he can't work any faster.

It is the existence of such transition cases and of the cæsural tendency in many 4-stress lines that justifies making one class of such divergent varieties.

After these in frequency comes a third four line stanza, composed of three 3-stress lines (the first, second and fourth) and one 4-stress line (the third). Like the last this form occurs in two varieties, one with internal rhyme in the 4-stress line and one without. For example,

Hickory, dickory, dock.
The mouse ran up the clock.
The clock struck one; the mouse ran down.
Hickory, dickory, dock.

and

Peas porridge hot,
Peas porridge cold,
Peas porridge in the pot,
Nine days old.

These correspond in pattern to the short meter stanza of the hymn book, and, when taken as composed of two long lines, an Alexandrine and a septinary, to the "poulter's measure" of the Elizabethan writers. Schipper (*op. cit.*, p. 199) regards it as better suited to comic verse than verse of other sorts, and single stanzas of the variety with internal rhyme (the 4-stress line being printed as two twos) were very common in the humorous columns of the newspapers a few years ago.[1] An interesting feature of the nursery rhymes of this pattern is that in most cases, at least of the more familiar ones, the last line is a repetition of the first, or a close approximation to it. The repetition seems to bring the whole to a close and round it off, like the return to the keynote in a piece of music.

The three groups of stanza patterns so far considered include about four-fifths of the nursery rhymes of the hundred in question, and nearly half the hymns of a hymnal examined for comparison.[2] Of the remaining rhymes some are of regular but infrequent forms (stanzas of 2- and of 3-stress lines, or of three 4-stress lines followed by a 3-stress line), some vary in form from stanza to stanza, and a few defy classification.

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[1] A single example will suffice:

A maiden, named Molly Maguire,
Had trouble in lighting her fire.
The wood being green,
She used kerosene;
She has gone where the fuel is drier.

[2] The revised edition of that used in the Episcopal church in this country.

Of the three more common patterns, the first has less definiteness and unity than either of the others, or depends for its unity to a considerable degree upon other than rhythmic conditions. When the four line pattern is composed of couplets, there seems little reason in the rhythm alone why it should not stop at the end of the first couplet, or go on indefinitely, couplet after couplet, as indeed it does in some cases. When the pattern is represented in bare taps (*e. g.*, with a lead pencil on the table) there seems also little reason for stopping after four groups of four taps rather than after two such groups or even after one. The unity of the couplet itself is not great, though in setting over against each other two natural rhythmic units (here two 4-stress groups) it may have a certain degree of completeness, and this may be further strengthened by the ease with which a 4-stress couplet fits into the ebb and flow of respiration, but the presence of rhyming words is important in marking off both the couplet and quatrain of this sort. Upon what other factors their rhythmic importance depends remains to be investigated.

The second pattern has distinctly more unity than the first. The 4-stress line, or the pair of 2-stress lines, with the 3-stress line following forms a whole having a definite auditory configuration, and is recognizable when reproduced in bare taps. Two such wholes constitute the stanza, which thus acquires a unity comparable to that of the couplet.

The third pattern is the most distinct and unitary of all. It comes to a definite close and has no tendency to repetition, except as a whole, a characteristic which perhaps has something to do with its fitness for single stanza humorous verses and its unfitness for general lyric use where all the stanzas must contribute toward the larger unit of the poem. It seems to stand on the same grade of unity as the single 4-stress line of the first group and the half stanzas of the second group. When represented in bare taps, it has a perfectly clear and recognizable character, and has often been used, as will be shown later, where definite and recognizable rhythmic effects are desired.[1]

The essential thing in all these stanza patterns appears to be the stressed syllable, or rather the group of stressed syllables recurring, at equal intervals of time, and marked off, group from group, by pauses or rhymes or both. The syllables which fill up the intervals between the stressed ones, introduce them, or follow them, seem generally to be of less importance. They make one stanza differ from another of the same pattern, but they do not change the pattern. The pattern on the contrary seems to dominate the syllables and, at least in the nursery

[1] *Cf.* Section III below.

rhymes, to force them into accord with itself. Many examples of this are to be found, especially where several syllables are crowded into a single interval, or where a single stressed syllable is extended to fill a whole one. Some of these will be noticed in the experimental section below; here we desire to speak of two or three rhymes in which, with different treatment of the syllables in question, conformity to more than one pattern is possible. In the following rhyme, for example, the first and third lines are a syllable short for the common meter pattern.

Sing a song o' six pence,
A pocket full of rye;
Four and twenty blackbirds,
Baked in a pie.

If the last syllable in these lines is unstressed, the stanza pattern becomes one of four 3-stress lines—a rare one among nursery rhymes. If on the contrary the syllable immediately preceding the last is held long enough to bring a stress on the final syllable it conforms to the common meter pattern, and this as a matter of fact appears to be what often happens. The results of tests with children on this point will be given in the next section. Two lines in other stanzas of the same rhyme have the full quota of syllables, though this alone would not be conclusive; for the rhymes do not always adhere to the same pattern throughout. It is probable also that some readers bring the words to a familiar pattern in another way, namely, by taking the rhyme as a tetrameter couplet, which would make it conform to the still more common 4-stress group. Thus,

Sing a song o' six pence, a pocket full of rye;

Four and twenty blackbirds, baked in a pie.

In the following rhyme (which, however, did not occur in our hundred) there is an extra syllable in the second and fourth lines, if the rhyme is taken as having the common meter pattern, or a syllable too few if it is taken as having the long meter pattern.

Once in my life I married a wife,
And where do you think I found her?
On Gretna Green in a silken sheen,
And I took up a stick to pound her.

She jumped over a barberry bush,
And I jumped over a timber,
I showed her a gay gold ring,
And she showed me her finger.

In the first stanza the syllables immediately preceding the last in these lines—"pound" and "found"—are of them-

selves very long, and a little further time is also required if the h in "her" is given distinctly—so that it is easy to make these syllables long enough to bring an extra stress on the following syllable and fit the whole to the pattern of four 4-stress lines. In the second stanza the penultimate syllables of the second and fourth lines are not so long and the tendency to stretch the lines is less strong, and perhaps would not be sufficient to keep the rhyme from falling into the common meter pattern, were it not for the influence of the stanza immediately preceding.[1]

In the similar rhyme beginning "See, saw, Margery Daw," the penultimate syllables of the second and fourth lines are only of moderate length, and the tendency to extend the lines to the 4-stress limit is not very marked. It is probable, however, that even this rhyme is sometimes pressed by children into the pattern of four 4-stress lines. In a musical work entitled "Mother Goose Songs without Words,"[2] in which a skillful musician has tried to catch the rhythm of familiar nursery rhymes as they are repeated by children, both "Sing a song o' six pence" and "See, saw, Margery Daw" are fitted with notes that indicate 4-stress lines. The same is true also of "Bye, Baby Bunting," and "Goosey, goosey, gander." The majority, however, of the group of school children tested with the former of these rhymes, gave it but three stresses per line.

Little need be said about the foot structure of the nursery rhymes, except that they are extremely free, the only requirement seeming to be that the stressed syllables should recur at approximately equal intervals. As a result of this liberty the three syllable feet are freely mixed with the two, and it is rare to find a rhyme that maintains a single type of foot throughout. Some apparent irregularities in the way of extra syllables at the beginning of lines will, however, find their proper place in the rhythm when the rhymes are written in long lines instead of short.

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[1] Brücke in his *Physiologische Grundlagen der neuhochdeutschen Verskunst*, pp. 25 f, finds a similar result with Goethe's *Es war ein König in Thule*. When the recitation of such verses is accompanied by tapping movements of the hand, as in Brücke's experiments and in those to be discussed in the next section, the tendency is very strong to force the rhythm, natural pronunciation of the words being a little strained in order to fit the more uniform movement of the hand. Something similar seems to have been observed by Pringle. *Cf.* Shaw and Wrinch, A Contribution to the Psychology of Time. University of Toronto Studies, No. 2, p. 50.

[2] By Mrs. L. E. Orth, published by The Oliver Ditson Co., Boston, 1897. The composer has endeavored by a series of simple pieces embodying the familiar rhythms of the nursery rhymes to pave the way for children to a comprehension of rhythms as expressed in the ordinary musical notation.

It would be an interesting question, finally, to ask how it happens that the three chief stanza patterns hold the place they do among the nursery rhymes and in serious verse. It is easy to say, and doubtless in large measure true, that they owe their unique position now to their great frequency in the past. We have been trained to them as we have to the ordinary church cadences and the return to the keynote, but the question remains of why they came into frequent use originally, and the answer must probably be looked for outside the data obtainable by a study of the forms themselves.

II. An Experimental Study of the Rhythms of Nursery Rhymes.

Purpose and Method. Our purpose in undertaking an experimental study of these rhymes was one that must have suggested itself to every student of rhythm, namely, to secure, if possible, some objective record of rhythms as they actually occur in spoken verse.

The way in which we finally approached the question, after much preliminary testing, was essentially like one of those used by Brücke, though improved, as we believe, in some particulars.[1] In both methods the rhythm was tapped by the finger

[1] Brücke: Die physiologischen Grundlagen der neuhochdeutschen Verskunst, Wien, 1871, pp. vii, 86. His methods were at the time unknown to us.

A few words with regard to the methods of Brücke and others may be added here. Brücke made use of two methods, the first for studying the relations of the stressed syllables in the line or stanza, the other for determining the quantities of the syllables in the foot. The first (*op. cit.*, p. 23) consisted in marking each arsis, or each arsis carrying an ictus, with a quill on an evenly revolving kymograph drum, while reciting verse of different kinds. In the second method (p. 32, ff.) the lip movements were recorded during the pronunciation of certain lines specially constructed in syllables involving labial consonants to show characteristic foot structures. The lip movements were traced by means of a thin strip of wood, fast at one end, and resting at the other on the speaker's lip. At a suitable place a writing point was attached for inscribing on the drum surface.

Hurst and McKay (Experiments on the Time Relations of Poetical Meters, University of Toronto Studies, Psychological Series, No. 3, 1899) were, unknown to us, at work at about the same time on a closely related problem and by a similar method, a refinement of Brücke's first method. Their plan was to have the syllables of ten or twenty successive feet of standard selections marked while the subject scanned them silently, or to have series of feet of the standard forms (dactyl, anapest, etc.) tapped off empty, *i. e.*, without words, while attention was concentrated on the rhythm. It is important to note that in using the tapping method for the marking of the constituent elements of the foot instead of the interval from arsis to arsis the work of Hurst and McKay approaches that of Brücke with his second method, while ours is like that by his first method.

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in accompaniment to the voice reciting the lines, but in ours a voice record also was taken by one of the methods used by Rousselot and more recently by Bourdon.[1] For the finger record a small receiving tambour, placed conveniently to hand on the table, received the taps and communicated them to an inscribing tambour which in turn transferred them to the smoked surface of a kymograph drum, each tap being marked by a sharp departure of the pointer from the base line. For the voice record a short tube of large diameter fitted with a suitable mouth-piece led directly into the metal box of a tambour, the stylus of which traced on the drum the impulses communicated to it by the breath of the speaker. For a time line an interrupter giving tenths of a second was used, and the speed of the drum was such as to allow the reading of these by estimation in tenths, making the unit of measurement 0.01 second.

Records were taken during the course of the experiments upon a number of different subjects, but in small number, and only those taken upon the writers will be considered here, except in the case of certain tests made upon school children. Eleven rhymes were studied sufficiently for report. Five of them were of the long meter or other 4-stress patterns, as follows:

Georgie, porgie, pudding and pie,
Kissed the girls and made them cry.
When the boys came out to play,
Georgie, porgie, ran away.

Intery, mintery, cutery, corn,
Apple seed and apple thorn;
Wire, brier, limber, lock;
Three geese in a flock;
Sit and sing by a spring;
O, U, T, and in again.

Diddle, diddle, dumpling, my son John
Went to bed with his stockings on;
One shoe off and one shoe on,
Diddle, diddle, dumpling, my son John.

There was an old woman, and what do you think,

VII, 1899, pp. 95, ff.) has made an exhaustive study by refined methods of the first stanza of "Who Killed Cock Robin?" but as the rhyme was apparently read rather than scanned and is one of the more complicated ones metrically, his study and ours do not overlap to any considerable extent.

These researches are the only ones known to us, if we except the brief account of results given by Meumann, in which the particular matter in hand has been approached in an experimental way. The results reached, so far as they have to do with our own, will be spoken of in the text as occasion arises.

[1] Bourdon: L'Application de la méthod graphique à l'étude de l'intensité de la voix, *L'Année psychologique*, 1897, 369-378.

She lived upon nothing but victuals and drink.
Victuals and drink were the chief of her diet,
And yet this old woman could never keep quiet.

There was an old woman that lived in a shoe;
She had so many children she did n't know what to do.
So she gave them some broth without any bread,
And whipped them all soundly and sent them to bed.

Those of the second, or common meter, class were:

See, saw, Margery Daw;
Johnny shall have a new master.
He shall have but a penny a day,
Because he can't work any faster.

Mistress Mary, quite contrary,
How does your garden grow?
Silver bells and cockle shells,
And pretty maids all in a row.

Old King Cole was a merry old soul,
And a merry old soul was he.
He called for his pipe and he called for his bowl,
And he called for his fiddlers three.

If all the world were apple pie,
And all the sea were ink,
And all the trees were bread and cheese,
What should we do for drink?

Hey! diddle, diddle, the cat and the fiddle;
The cow jumped over the moon.
The little dog laughed to see such sport,
And the dish ran away with the spoon.

The single example of short meter was

Hickory, dickory, dock,
The mouse ran up the clock;
The clock struck one, the mouse ran down.
Hickory, dickory, dock.

Our main dependence in the working up of the combined hand and voice records has been that of the hand. The means employed for recording the voice are imperfect, really furnishing a record of the movements of the breath and not that of the vocal intensities at all. From such a record alone it would therefore be quite impossible to get any picture of the rhythm. The voice record, however, does enable us, at least in the case of the explosive consonants (*p*, *b*, *k*, and *c* and *g* hard) to determine with some exactness how nearly the finger tap agrees with the vocal stress.[1] That the voice and hand should keep in close coincidence was of course to be expected in view of the strong natural tendency to beat time in some

[1] Brücke, *op. cit.*, p. 24, f., and 32.

way or other, but an objective determination of the degree of exactness is not wholly without interest. The relation between the hand and voice has been carefully worked out for both subjects with the rhyme "Georgie, porgie," which is rich in these consonants. In few cases does the average difference between the hand and voice (average of ten trials) exceed 0.02 sec. Once in the case of S with the word "boys," the difference reaches 0.049, but this is so much larger than the rest that an error is probable either in locating the point of the voice curve corresponding to the beginning of the *b* or in supposing that in the enunciation of this subject the stress in this syllable falls close to the consonant and not later in the diphthong following. The other cases all lie below 0.03. In the record of S for "Mistress Mary" the same relations are true, except for the first syllable of "pretty" where the average difference is 0.022, and for "bells" where it is 0.031. When the mean variations are examined it is found that in "Georgie, porgie" they exceed 0.02 in two cases, once for each subject, the highest being 0.03 sec. In "Mistress Mary" for S there are also two instances the highest being 0.026. The differences of hand and voice are larger than could be wished, yet, even if we take the extreme cases (excluding only that of S with the word "boys") we find that they rarely exceed the limit laid down by Meumann (0.02);[1] and if we take in the same way the largest mean variations, we find that none correspond to a probable error in the averages of ten of more than 0.01 sec. There would, therefore, appear to be sufficient justification for taking the finger record, within the limits indicated, as synchronous with the vocal stress.

It should not be inferred that the tapping introduced a distraction into the recitation of the rhymes, for this was not the case. From the first the tapping was practically automatic, and no difficulty at all was found in doing both at once. The effect of the tapping, as has been intimated in the preceding section, was probably of quite another sort, tending to enforce by its own regularity, a greater steadiness on the recitation than would usually be expected in vocal utterance. In the case of genuine poetry this would certainly be a serious, and, we believe, a fatal, objection to the method, but in the case of the nursery rhymes, where the usual recitation often approaches scanning, the greater regularity, if introduced, would seem to be a secondary matter. Nevertheless the fact should not be neglected in inferences based upon the results to be given. In taking the records the rhymes were recited in the

[1] Untersuchungen zur Psychologie und Æsthetik des Rhythmus, Wundt's Studien, X, 1894, p. 419.

child's fashion, making the rhythm prominent, nearly to the full extent of scanning.

General Rate of Recitation. A question that naturally arises in discussions of rhythm is whether or not there is anything like a fixed individual rhythm to which the reading of each subject tends to conform itself. The answer which our experiments give to this question is partly affirmative and partly negative. The subject S read more rapidly than T with nine out of the eleven rhymes used, and often by a very considerable amount, and other experiments have shown a similar tendency in his reading of other sorts of verse. There would thus appear to be a certain relative constancy of type—quick, moderate or slow—in the rate selected, but not one by any means that excludes differences, *e. g.*, between repetitions of the same rhyme on different occasions, especially with different mental conditions, between different rhymes of the same metrical pattern, or between rhymes of different pattern. This will be clear from the following table in which are given the average foot or more exactly the average interval between arsis and arsis for the different rhymes studied. In calculating this all partial feet and all feet affected by the regular rhythmic pauses have been excluded.[1]

TABLE I.

Showing in seconds the average interval between stresses.

LONG METER PATTERN.	S.	T.
Georgie, porgie,	0.458	0.516
Intery, mintery,	0.548	0.566
Diddle, diddle, dumpling,	0.471	0.527
There was an old woman, and what do you think?	0.476	0.568
There was an old woman that lived in a shoe,	0.652	0.620
COMMON METER PATTERN.		
Mistress Mary,	0.503	0.562
Old King Cole,	0.522	0.616[2]
Hey! diddle, diddle,	0.322[3]	0.458[4]
See, saw, Margery Daw,	0.548	0.533
If all the world were apple pie,	5.406	0.484
SHORT METER PATTERN.		
Hickory, dickory, dock,	0.361	0.452

[1] The averages are given in this case without the mean variations, which would have required an amount of extra figuring quite disproportionate to the additional value afforded by them. Their size can be judged with sufficient exactness from those given in Table II, where the intervals are presented in percentages with their corresponding mean variations.

[2] Average of 9 repetitions.

[3] Average of 7 repetitions.

[4] Average of 8 repetitions.

An examination of this table will show the individual differences in rate above referred to and also differences depending on other causes. It is evident that the character of the rhyme itself is decidedly important. Both subjects agree in the main in the relative rates at which they take the rhymes, as appears in the following columns where the rhymes are arranged in their order of rate from the slowest to the quickest.

Subject S.	Subject T.
There was an old woman that lived in a shoe.	There was an old woman that lived in a shoe.
See, saw, Margery Daw.	Old King Cole.
Intery, mintery.	There was an old woman, and what do you think?
Old King Cole.	Intery, mintery.
Mistress Mary.	Mistress Mary.
There was an old woman, and what do you think?	See, saw, Margery Daw.
Diddle, diddle, dumpling.	Diddle, diddle, dumpling.
Georgie, porgie.	Georgie, porgie.
If all the world were apple pie.	If all the world were apple pie.
Hickory, dickory, dock.	Hey! diddle, diddle.
Hey! diddle, diddle.	Hickory, dickory, dock.

Just what caused these differences in rate of recitation is not altogether clear. The number of syllables, their absolute quantity, and the phonetic labor involved in them are probably important factors, but may be overpowered in particular cases by others. It seems possible that the syllables of the first foot or two by their quick or slow movement may set the rate for the rhyme as a whole, making "Hey! diddle, diddle," for example, rapid, and "Old King Cole" slow. Familiarity may also influence the rate, for though all the rhymes used were familiar, some had progressed further toward complete automatism than others. And lastly the images suggested by the rhymes—especially those of movement may have an effect—as, for example, in the case of S with "See, Saw, Margery Daw" where the slow movement suggested by the rising and falling "see-saw" very possibly set the time for the whole. Whether the general emotional tone is sufficiently different to exercise an independent influence is so far a matter of conjecture.[1]

Relative Length of Intervals in Different Portions of the Line and Stanza. In order to present these differences unobscured by variations due to causes lying outside the rhyme itself, it

[1] It has been suggested that the similarity in the relative rates given to the various rhymes by the two subjects was due in part to unconscious imitation, for each subject heard the other recite. This may be possible; but in view of the decided difference in absolute rate between the subjects, and the different rates given to "See Saw" and "There was an old woman, and what do you think?" we are not inclined to attach very much importance to it.

has seemed better to give the foot lengths (intervals between stresses as marked by the finger taps) in percentages of the total time required for reciting the rhyme, instead of in the time values obtained directly from the kymograph sheets.[1] These percentages with their corresponding mean variations are given in the several parts of Table II inserted at the end of this section. Each figure is the average of ten percentage records (except where the contrary is specified) calculated from the original time values. The average recitation time in seconds is given for each rhyme immediately following the initial of the subject, and from this the actual time of any interval can be calculated approximately should any one desire to do so. To these have been added a few records of tappings of the three standard patterns without recitation of any rhymes whatever, simply as pure metrical patterns. Little importance was attached to these when they were taken, and they are therefore very few in number, but they are so consistent with themselves and have proved so interesting in connection with the records of the rhymes that it has seemed worth while to publish them. These records are given complete in units of 0.01 sec.

In discussing this table we shall consider first those general features which throw light upon the recitation of all the rhymes, then those that have to do with the different stanza patterns and individual rhymes, and finally such indications as appear with reference to the rate of recitation of feet of various forms.

It will be noticed that there is a rough equality in the figures

[1] The "total time" referred to in the text was measured from the beginning of the first finger tap to the end of the voice record, except in the case of the rhyme "Mistress Mary" with subject S, where the end of the voice record could not be determined with sufficient accuracy, and the time used was that between the first and last finger taps. The record does not include the time of introductory syllables such as occur in three of the rhymes, which were not easy to measure with certainty by the method employed. In the case of these rhymes the record is therefore incomplete, though not to a degree to invalidate it in any way for the purposes to which we shall apply it. The part of the voice record following the last finger tap was secured by having the subject take breath instantly after the enunciation of the last syllable while his mouth was still in contact with the mouthpiece. This introduced a sharp and unmistakable fall in the voice tracing which was taken as marking the end of the recitation. The determinations thus obtained are a little too large, but probably by an insignificant amount—less we believe than the usual reaction time. The criticism may possibly be offered that the effort to mark the end of the rhyme in this way may have led to a slight quickening of the last foot or two. We regret that we cannot at present furnish absolutely conclusive evidence to the contrary, but as the taking of the breath at that point soon became automatic, we are not inclined to think that the length of the adjacent intervals was seriously affected by this cause.

for the intervals except where they are lengthened by pauses, as in the seventh interval of the common meter pattern and in the third and sixth of the short meter pattern, or where the figure stands for a terminal syllable, as is the case with the last figure given for each rhyme. This is in accord with Brücke's generalization (*op. cit.*, pp. 23, f.) and the results of Hurst and McKay (*op. cit.*, p. 66), but it is also clear that the uniformity is only an approximate one, and subject to considerable variations. One of the most striking of these is a progressive quickening in rate from first to last of the recitation. This occurred with S in all of the rhymes, and with T in all but two, as appears in the following table in which are set over against each other the average percentages for complete intervals in the first and second halves of the rhymes.

TABLE III.

Showing average percentages for complete intervals in the first and second halves of the rhymes.

RHYMES.	SUBJECTS.	FIRST HALF.	SECOND HALF
Georgie, porgie,	S	6.57	6.19
	T	6.63	6.25
Intery, mintery,	S	4.29	4.18
	T	4.25	4.15
Diddle, diddle, dumpling,	S	6.44	6.23
	T	6.50	6.37
There was an old woman, and what do you think?	S	6.39	6.19
	T	6.57	6.23
There was an old woman that lived in a shoe,	S	6.67	6.23
	T	6.54	6.23
Mistress Mary,	S	7.38	7.07
	T	7.10	7.00
Old King Cole,	S	6.95	6.75
	T	7.38	7.07
Hey! diddle, diddle,	S	6.78	6.75
	T	6.95	7.15
See, saw, Margery Daw,	S	7.11	6.30
	T	7.10	6.53
If all the world were apple pie,	S	6.78	6.51
	T	6.95	6.85
Hickory, dickory, dock,	S	7.30	6.84
	T	7.68	7.86

The only rhymes for which this relation does not hold are "Hey! diddle, diddle," and "Hickory, dickory, dock," where it fails for T. A portion of this difference comes in some cases from an unusual length in the initial interval and an unusual brevity in the last, but the whole tendency is not to be explained in this way. The progressive decrease in time appears even when the rhymes are taken line by line. The averages of the feet

in the successive lines of the rhymes of the 4-stress group are given in Table IV. The facts revealed by them are also true of those of the other patterns.

TABLE IV.

Showing average percentages for complete intervals in the successive lines of rhymes of the 4-stress group.

RHYMES	Subjects	1st Line	2nd Line	3rd Line	4th Line	5th Line	6th Line
Georgie, porgie,	S	6.83	6.30	6.10	6.10	—	—
	T	6.90	6.37	6.43	6.07		
Intery, mintery,	S	4.33	4.13	4.23	4.17	4.23	4.00
	T	4.30	4.10	4.23	4.07	4.27	4.03
Diddle, diddle, dumpling,	S	6.53	6.23	6.26	6.06	—	—
	T	6.70	6.26	6.53	6.23		
There was an old woman, and what do you think?	S	6.60	6.20	6.16	6.23	—	—
	T	6.86	6.40	6.36	6.20		
There was an old woman that lived in a shoe,	S	6.96	6.33	6.36	6.10	—	—
	T	6.70	6.50	6.30	6.13		

Reasons for either the general acceleration or the long initial and short terminal intervals are not very obvious. A variety of more or less plausible conjectures offer themselves. There is possibly a certain amount of inertia in both hand and vocal apparatus at the start, which gives place, as the tapping and recitation progress, to an increasing facility of action due to "warming up" or a local practice, or to the mild excitement of making the experiment.[1] Relatively careful beginnings with rapid and careless endings are not uncommon elsewhere. Observation of a person reading prose aloud will reveal a similar tendency. While the idea is rising into consciousness the words are pronounced slowly and with em-

[1] For the effect of moderate excitement on tapping rates see Dresslar, Some Influences which Affect the Rapidity of Voluntary Movement, *American Journal of Psychology*, IV, 1891-92, p. 523; also Nichols, The Psychology of Time, *Ibid.*, p. 83.

One of the writers believes that the greatest cause of change probably lies in the relation to consciousness of the words in which the rhyme is clothed. The apperceptive effort is high, and the reading rate slow, at the beginning of the stanza and of each phrase, but the effort declines, and a corresponding increase of the reading rate occurs, when the main work of comprehension is for the moment over. This would be but another instance of the general law of economy in accordance with which all our activities tend to be performed with the least possible expenditure of nervous energy.

phasis; having reached the focal point of the idea, however, the remainder of the sentence is hurried out of the way in a more or less indiscriminative mumble. Closely connected with this, doubtless, is the prominence which, according to the rhetoricians, belongs to the first part of a sentence or phrase, for slowness is a normal mark of emphasis. In rapid writing, also, sufficient words of a sentence to carry the meaning are written legibly, the rest less perfectly. A long word may end in a straight line, the writer trusting to the opening syllable and the context to make the thought understood. That the causes are general rather than special is probable from the fact that the same relations hold in some cases, at least, when the patterns in question are executed in bare taps (*cf.* Table II, Pt. v, on the last of the inserted sheets at the end of this section), and similar tendencies have been found even in uniform tapping under certain conditions by experimenters upon time judgments who have made use of the tapping method.[1]

If Table II is examined with reference to the stanza patterns, it will be found that certain of the intervals show characteristic differences. The most striking of these are those which mark off the half stanzas in the common meter pattern and the first two 3-stress lines in the short meter pattern. These are more marked in the case of S, but appear in the records of both subjects, even when the patterns are given in bare taps. In the common meter pattern the interval is often nearly or quite double that of the ordinary intervals. In the short meter pattern its excess ranges from about one-seventh to more than two-thirds of the intervals on either side. The interval in these cases is taken up partly with the completion of the enunciation of the stressed syllable immediately preceding (what proportion can be roughly estimated from that of the last syllable of the rhyme), by a pause, and by the introductory syllable of the following stress when there is one. Such pauses are often used also for taking breath. In the long meter pattern there is also a tendency to mark the line groups by lengthening the interval between the final stress of one line and the initial stress of the succeeding one, but by no means so pronounced. If the general average for this group is consulted (Table II, Pt. i), it will be found that S invariably lengthens these intervals (and also shortens the first interval of the new line). The amounts in question are very small, but the relation can be traced in most of the averages for the single rhymes of the group and is clear in the records of the bare tapping (*cf.* Pt. v). The same relation appears in T's record with bare taps and in some of the averages

[1] Nichols, *op. cit.*, p. 82.

for the single rhymes, but does not appear in the general average and fails distinctly in some of the single rhyme averages. In some cases, however, it may possibly be hidden or neutralized by T's tendency to lengthen the first interval of the line.

The smaller rhythmic periods—couplets and lines—are not further divided by measurable lengthening of special intervals (except as already noticed in the case of the first two 3-stress lines of the short meter pattern), not even in the case of 4-stress lines with internal rhyme. In the bare tap rhythms, however, there are traces of such a method of demarcation (*cf.* Pt. v).[1] The general tendency to increase of rate within the subordinate groups is, however, not without an influence. It gives to the 3-stress lines in the common meter pattern a quicker movement than the 4-stress lines, and may thus be partially responsible for the "falling" character which they seem to have. The subjects differ so much in their rates for the second half of the short meter pattern that conclusions cannot be drawn from the table as to its movement.

It remains to speak of the size of the intervals in relation to foot structure, in other words to the nature of their syllable contents. In the rhymes used there is a considerable variety in this respect, ranging from intervals containing but a single syllable, as in the first two of "See, saw, Margery Daw," to those containing four as in the fifth and seventh of "There was an old woman that lived in a shoe" or the first of "Diddle, diddle, dumpling." Yet few differences are apparent in the figures that can be referred directly to this cause, a result to be expected, perhaps, in view of the prominence given to the rhythm in reciting. It cannot be doubted that when adult verse is read for its meaning as well as metrical structure, very material differences in intervals are to be found. There is reason, indeed, to think that the overfull intervals in "There was an old woman that lived in a shoe" break into the regular movement of the pattern as the rhyme is ordinarily recited by children, though, as will presently be shown, they can bring it into complete uniformity when necessary.

Three of the rhymes—the two about the old women and "If all the world were apple pie," are in iambic-anapestic movement—the last in perfect iambs throughout. The remaining eight are trochaic-dactylic. It does not appear, so far as our results go, that these differences affect the length of the

[1] Such a difference between the verse rhythm and that of bare taps is not unexpected, for the tapped rhythms, having to depend chiefly upon quantity, are at a disadvantage compared with those in vocal form which have at their disposal rhyme, pitch and intensity in addition.

intervals or the general rate of recitation in any uniform way. It is not so much in the general rate of the line as at the beginning and end, where the presence or absence of an extra syllable makes the difference between an abrupt or a gradual beginning, or ending, or sets the character of the movement for the whole that these prosodic differences are effective; though there may also be a difference of movement within the interval, if Hurst and McKay are correct in their measurements of the short syllables in the theses of dactylic and anapestic feet. These authors report characteristic differences in rate for the different feet and conclude that "the dactylic foot tends to be shorter than the anapestic and the trochaic than the iambic" (*op. cit.*, p. 66, f.). This, however, runs counter to common observation, at least with reference to the anapest,[1] and does not hold in our experiments on the rhymes. In Table I, for example, it appears that the pure iambic rhyme "If all the world were apple pie" has a shorter average foot than "Georgie, porgie," which is almost exclusively trochaic. The same table also shows that rhymes containing the same sorts of feet may differ very widely in their rates of recitation. We cannot but think that these authors have been led into error in this particular. A very large number of experiments would be necessary to establish such a difference as they describe; for even the difference between dactyls and trochees and between anapests and iambs disappears when they occur in the same line and the line is scanned.

Observations on the Rhythm of Nursery Rhymes as Recited and Tapped by Children. As a supplement to the experiments above reported, and particularly as a test of the manner in which some of the rhymes which do not quite fit the ordinary patterns would be handled by the children themselves, one of us undertook the observation of a number of young children in one of the public schools of the city.[2] Over forty children of an average age of ten years were tested in all. They were taken singly and, without any suggestion, required to recite several of the rhymes, tapping the rhythm at the same time with the finger. As they are accustomed to do something similar in the counting-out rhymes and in music, they found no difficulty in doing so. The results may be summarized as follows:

Of fifteen children asked to tap the rhythm in "Bye Baby

[1] Coleridge, for example, in the familiar poem in which he illustrates the common forms of feet characterizes them particularly as "the swift anapests."

[2] It is a pleasure to acknowledge here our indebtedness to the Worcester school authorities and to Miss Ella L. Dwyer, the principal of the Oxford St. School, for the opportunity of testing these children.

Bunting" four gave four accents to the line, ten beat it in three and one in two beats. "Diddle, diddle, dumpling" was given in the common meter pattern by nine out of sixteen children. Seven others gave it differently, chiefly in three stress lines, all or nearly all failing to take the somewhat more difficult adaptation to the long meter pattern that we ourselves used in the measurements above reported. Forty-four children recited the familiar rhyme, "Sing a song o' six pence." Of these twenty-seven gave four stresses to the first and third lines, four gave them three stresses, and thirteen two stresses. It was found that the children were satisfied with any of these simple rhythms, and if asked to change did so readily. The fourth line of "Mistress Mary"—"And pretty maids all in a row"—which contains the long syllable "maids" where a short one would be expected, was given in regular time by nineteen children and irregularly by nine. There seemed to be little difficulty where the rhyme was well known. In "There was an old woman that lived in a shoe" the line "She had so many children she did n't know what to do," which contains two feet with supernumerary syllables was taken rhythmically by thirty-three and irregularly by seven children.

A further test was made by placing before each child in his turn a card on which was written

1	2	3	4	5
I	caught	a	hare	alive
6	7	8	9	10
I	let	it	go	again

and requesting him after reading it to himself to read it aloud. Two-thirds of the children read the numerals rhythmically, putting the stress on the 1, 3 and 5 of the first line and 6, 8 and 10 of the third, and in nearly all cases strengthened the accent on repetition.

These tests give evidence both of the facility with which children reduce recalcitrant lines to conformity with the rhythmical patterns and of a freedom in the selection of patterns in doubtful cases. How far the cases in which the rhymes were recited irregularly show habits of repeating them in that way cannot be said, but it is not unlikely that some rhymes are never forced into perfect rhythm by a considerable number of children. These satisfy themselves with the regularly rhythmed portions and get over the irregular portions in the easiest way they can. A more extended study of the rhymes as recited by children when saying them with perfect freedom, without the constraint of tapping or the presence of an unfamiliar observer, would be an interesting and important contribution to the subject.

The general results of this section of our studies may be summarized as follows:

1. We find, with previous investigators, a general uniformity in the intervals between stresses (except when they contain the pauses that mark off the larger rhythmic periods), but also that there is, in the rhymes as a whole and in the rhythmic periods within them, a tendency to a quickening in rate from first to last.

2. The characteristic movement of the most frequent patterns depends partly on the distribution of pauses and partly perhaps on this tendency to increase of rate.

3. The tests on school children show a general tendency on their part to force imperfectly worded rhymes into conformity with one or the other of the more common patterns, but also a certain freedom as to the pattern selected.

III. College Yells.

Perhaps in no field is the phenomenon of rhythm—pure rhythmic noise—more strikingly displayed than in the yells and cheers of college students. From the list published in the *World Almanac* for 1895 and various other sources a collection of fully two hundred college yells has been made, and these have been examined with reference to metrical pattern. While it is not possible in every case to tell from the printed text in just what rhythmic form the cheers are actually given, several points of considerable interest are apparent. The same metrical patterns are found as in the nursery rhymes, but in strikingly different proportions. Yells based on any of the pure 4-stress patterns or on the common meter pattern are infrequent, while yells conforming exactly to the short meter pattern are extremely common, forming as much as a quarter of the whole list, without including many imperfect examples. Several of the yells of the 4-stress group appear to follow the couplet pattern, others the pattern of three 4-stress lines followed by a more or less perfect 3-stress line, and few, if any, that of the long meter stanza.[1] Among the yells were also found several examples of a metrical pattern not represented at all in our list of nursery rhymes, though not wholly unknown to children. It consists of a two followed by a three. Here is an example: Rah, Rah! Rah, Rah, Rah! Rah, Rah! Rah, Rah, Rah! Swarthmore. This is evidently based on the drill sergeant's Left, Left! Left, Right, Left! In the political cam-

[1] One of the writers of this study is inclined to regard some of the cases here classed as 4-stress yells as really 2-stress yells. From the data at hand it is perhaps impossible to say whether the two 2's constituting the four are to be regarded as subordinate groupings or not.

TABLE II. Part i.

Giving the average total time in seconds required for the recitation of rhymes of the LONG METER *pattern together with the relative values of the intervals expressed as average percentages of the total times.*

	Av. total time.	Georgie,	porgie,	pudding and	pie;	Kissed the	girls and	made them	cry.	When the	boys came	out to	play,	Georgie,	porgie,	ran a-	way.
S.	7.198	7.1	6.8	6.6	6.7	6.2	6.3	6.4	6 6	5.8	6.0	6.5	6.7	6.4	6.3	5.6	4 5
M. V.	.112	.33	.30	.14	.13	.28	.26	.26	.20	.19	.24	.27	.37	.32	.29	.23	.29
T.	8.036	7.4	6.8	6.5	6.6	6.2	6.4	6.5	6.4	6.1	6.8	6.4	6.3	6.3	6.2	5.7	3.8
M. V.	.183	.42	.23	.22	.29	.24	.15	.21	.20	.31	.22	.25	.24	.22	.23	.23	.25
		Diddle, diddle	dumpling,	my son	John	Went to	bed with his	stockings	on.	One shoe	off, and	one shoe	on,	Diddle, diddle	dumpling,	my son	John.
S.	7.414	6.4	6.6	6.6	6.7	5.9	6.3	6.5	6.7	6.3	6.4	6.1	6.6	5.7	6.4	6.1	4.8
M. V.	.160	.40	.21	.31	.18	.09	.20	.37	.18	.14	.18	.30	.24	.26	.34	.16	.42
T.	8.262	6.7	6.7	6.7	6.6	6.3	6.0	6.5	6.3	6.9	6.2	6.5	6.3	6.2	6.4	6.1	4.3
M. V.	.186	.29	.41	.20	.23	.24	.13	.11	.22	.29	.17	.36	.24	.20	.23	.27	.34
	There	was an old	woman, and	what do you	think? She	lived upon	nothing but	victuals and	drink.	Victuals and	drink were the	chief of her	diet and	Yet this old	woman could	never keep	quiet.
S.	7.493	6.9	6.8	6.1	6 3	6.2	6.3	6.1	6.5	6.0	6.4	6.1	6.1	6.3	6.3	6.1	4.7
M. V.	.140	.38	.31	.23	.19	.16	.35	.21	.18	.14	.27	.27	.33	.25	.29	.36	.31
T.	8.885	7.4	6.9	6.3	6.2	6.8	6.2	6.2	6 2	6.4	6.2	6.5	5.9	6.4	6.2	6.0	4.1
M. V.	.154	.48	.15	.15	.17	.19	.14	.21	.16	.26	.33	.21	.23	.23	.30	.33	.43
	There	was an old	woman, that	lived in a	shoe. She	had so many	children she	did n't know what to	do. So she	gave them some	broth with-	out any	bread, and	whipped them all	soundly and	sent them to	bed.
S.	10.057	7.5	6.9	6.5	6.7	6.3	6 5	6.2	6.4	6.7	6.3	6.1	6.1	6.3	6.4	5.6	2.8
M. V.	.181	.24	.27	.22	.23	.20	.17	.19	.18	.36	.22	.16	.22	.35	.21	.24	.25
T.	9.619	7.1	6.8	6.2	6.2	7.2	5.9	6.4	6.5	6.7	6.1	6.1	6.3	6.5	5.9	6.0	3.2
M. V.	.189	.28	.27	.37	.30	.28	.31	.21	.30	.23	.28	.36	.12	.26	.31	.34	.22
Averages of the percentage values of the intervals.	S. 6.98	6.78	6.45	6.60	6.15	6.35	6.30	6.55	6.20	6.28	6.20	6.38	6.18	6.35	5.85	4.20	
	T. 7.15	6.80	6.43	6.40	6.63	6.13	6.40	6.35	6.53	6.33	6.38	6.20	6.35	6.18	5.95	3.85	

TABLE II. Part ii.

Giving the average total time in seconds required for the recitation of rhymes of the COMMON METER *pattern, together with the relative values of the intervals expressed as average percentages of the total times.*

	Av. total time.	Mistress	Mary	quite con-	trary,	How does your	garden	grow?	Silver	bells and	cockle	shells, And	pretty maids	all in a	row.
S.	6.941	7.8	7.9	7.4	7.6	6.7	6.9	13.0	7.2	7.3	7.1	7.3	6.8	6.7	———
M. V.	.213	.31	.37	.20	.33	.14	.25	.91	.16	.24	.25	.32	.15	.24	
T.	8.013	8.0	7.0	7.1	7.2	6.8	6.5	12.4	7.6	6.9	6.8	6.7	7.1	6.9	3.5
M. V.	.386	.56	.44	.44	.26	.29	.30	1.08	.43	.45	.19	.27	.32	.43	.37
		*Old King	Cole was a	merry old	soul, And a	merry old	soul was	he. He	called for his	pipe and he	called for his	bowl and he	called for his	fiddlers	three.
S.	7.530	7.4	7.3	6.7	7.1	6.8	6.4	12.6	7.5	6.7	6.9	6.7	6.8	5.9	4.0
M. V.	.322	.24	.29	.42	.33	.21	.36	.94	.33	.20	.26	.30	.23	.41	.55
T.	9.406	8.6	7.4	7.2	7.3	6.9	6.9	8.9	7.2	7.6	7.1	7.2	6.9	6.4	3.7
M. V.	.267	.67	.27	.33	.14	.33	.39	.50	.24	.25	.27	.14	.40	.26	.29
		† Hey! diddle,	diddle, the	cat and the	fiddle, the	cow jumped	over the	moon. The	little dog	laughed to	see such	sport and the	dish ran a-	way with the	spoon.
S.	6.747	7.1	6.9	6.8	6.9	6.7	6.3	11.4	6.8	7.1	6.8	6.8	6.6	6.4	6.7
M. V.	.234	.15	.07	.14	.17	.14	.38	.62	.12	.23	.11	.11	.12	.28	.27
T.	8.068	7.7	6.9	6 5	6.7	7.0	6.9	10.0	7.4	6.9	7.1	7.4	7.0	7.1	4.8
M. V.	.154	.30	.33	.27	.13	.37	.21	.36	.35	.17	.22	.18	.26	.37	.43
		See,	saw,	Margery	Daw;	Johnny shall	have a new	master.	He shall	have but a	penny a	day be-	cause he can't	work any	faster.
S.	8.142	7.7	7.5	7.3	7.1	6.7	6.4	14.1	6.3	6.4	6.3	6.3	6.0	6.5	5.2
M. V.	.342	.43	.25	.36	.31	.32	.34	.38	.21	.30	.24	.21	.26	.21	.21
T.	7.785	7.9	7.2	7.4	6.5	6.8	6.8	12.7	6.7	6.7	6.3	6.0	7.1	6.4	5.0
M. V.	.278	.34	.36	.30	.32	.27	.32	.64	.14	.23	.22	.29	.32	.18	.39
	If	all the	world were	apple	pie, And	all the	sea were	ink, And	all the	trees were	bread and	cheese, What	should we	do for	drink?
S.	6.073	7.1	6.9	6.6	6.8	6.8	6.5	13.4	6.9	6.6	6.7	6.8	6.4	5.7	6.2
M. V.	.159	.24	.27	.35	.29	.20	.21	.78	.28	.19	.31	.38	.30	.26	.34
T.	6.983	7.4	7.2	6.9	6.9	6.5	6.8	11.3	7.2	7.1	6.5	7.1	6.6	6.6	5.3
M. V.	.133	.33	.27	.16	.22	.27	.34	.60	.30	.20	.31	.17	.24	.40	.51
Averages of the percentage values of the intervals.		‡S. 7.33	7.15	6.85	6.98	6.75	6.40	12.88	6.88	6.70	6.68	6.65	6.45	6.13	5.53
		T. 7.92	7.14	7.02	6.92	6.80	6.80	11.06	7.22	7.04	6.76	6.88	6.94	6.68	4.46

*Old King Cole, average of 10 repetitions for S. and 9 for T.
† Hey! diddle, diddle, average of 7 repetitions for S. and 8 for T.
‡ Average of four rhymes only, "Mistress Mary" being omitted because of the defect of the last syllables.

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TABLE II. Part iii.

Giving the average total times in seconds required for the recitation of rhymes of the SHORT METER *pattern together with the relative values of the intervals expressed as average percentages of the total time.*

	Av. total time.	Hickory,	dickory,	dock, The	mouse ran	up the	clock, The	clock struck	one the	mouse ran	down.	Hickory,	dickory,	dock.
S.*	5.012	7.8	7.4	11.1	6.8	7.3	11.9	7.2	7.4	7.0	7.7	6.9	6.8	4.7
M. V.	.199	.57	.29	.56	.37	.34	.66	.40	.50	.32	.35	.33	.22	.43
T.	5.844	7.6	7.7	9.0	7.6	7.3	8.4	7.9	8.5	8.0	8.0	7.4	7.4	5.3
M. V.	.200	.50	.25	.33	.47	.33	.27	.28	.43	.28	.25	.20	.21	.27

* Based on 6 repetitions for S.

TABLE II. Part iv.

The table gives the relative values of the intervals for the rhyme "Intery, mintery," (a three couplet rhyme of 4-stress lines) expressed as average percentages of the total times.

Average total time: S. 12.971, M.V. 0.205; T. 13.408, M.V. 0.200.

	Intery,	mintery,	cutery,	corn,	Apple	seed and	apple	thorn.
S.	4.5	4.3	4.2	4.6	4.1	4.2	4.1	4.5
	.10	.10	.19	.17	.12	.11	.12	.19
T.	4.6	4.2	4.1	4.5	4.3	3.9	4.1	4.4
	.19	.14	.13	.15	.15	.19	.19	.19
	Wire,	**briar,**	**limber,**	**lock;**	**Three**	**geese**	**in a**	**flock.**
S.	4.3	4.3	4.1	4.0	4.3	4.3	3.9	4.1
	.11	.19	.09	.13	.21	.06	.21	.16
T.	4.3	4.4	4.0	4.1	4.3	3.9	4.0	4.2
	.16	.34	.11	.19	.22	.24	.26	.23
	Sit and	**sing**	**by a**	**spring,**	**O. U.**	**T. and**	**in a-**	**gain.**
S.	4.5	4.2	4.0	4.7	4.0	4.2	3.7	2.8
	.19	.12	.10	.11	.16	.18	.14	.22
T.	4.5	4.3	4.0	4.4	4.4	3.9	3.8	2.7
	.22	.23	.09	.16	.11	.15	.16	.24

TABLE II. Part v.

Giving in approximate thousandths of a second the values of intervals when the three most common stanza patterns are executed in bare taps. Timed with a tuning fork giving 99 v. d. per second.

Long Meter Pattern.

S.	295	314	324	330	270	282	300	315	284	282	295	314	276	293	273	—
"	315	305	270	330	300	285	314	340	263	285	285	332	280	280	285	—
T.	330	315	297	332	305	297	296	310	290	283	300	320	283	282	290	330
"	305	304	282	307	275	304	265	318	304	295	287	317	292	290	290	308

Common Meter Pattern.

S.	272	298	282	310	272	290	—	280	275	270	274	275	280	—
"	290	305	270	283	290	282	—	270	270	290	277	260	290	—
"	317	285	260	300	268	262	—	268	270	265	295	252	252	—
T.	—	—	—	—	—	—	—	310	320	304	324	303	310	495
"	323	305	295	335	310	302	520	290	295	305	300	300	305	540
"	312	301	295	310	300	288	545	310	293	293	327	332	290	583

Short Meter Pattern.

S.	285	298	496	280	298	475	275	274	246	280	266	280	—
"	292	290	475	270	275	472	253	283	255	285	255	280	—
T.	310	340	520	340	300	500	310	317	282	292	290	290	520
"	315	310	550	300	290	505	287	307	284	300	275	305	557

NOTE.—The blanks at the end of the records of S indicate that he paused a considerable time after the completion of the pattern, while T continued; the blanks in the middle of the common meter pattern, also indicate long pauses not counted. The tapping seems to have been executed by grouping sevens, rather than as a unitary stanza pattern.

paign of 1884 this pattern was used with the words "Blaine, Blaine, James G. Blaine" and a stamping accompaniment, an idea which Charles Ledyard Norton[1] says can be traced back to the Columbia College students. The same pattern has been observed in use by children as a march rhythm: January, February, March, March, March. The most striking difference, however, is the greater prominence of patterns involving 3-stress groups. These occur of course as constituent parts of both yells and nursery rhymes in the common and short meter patterns, but also among the yells in about eight out of nine of the remaining cases.

These college yells are the settled choice of the students of the various institutions to which they pertain, and in many cases are decided upon by vote of the student body. The proportions found may therefore be correctly considered as the rhythmic preferences of American college students for the purpose for which they are used. It is difficult to state the causes inspiring this choice. It is not to be taken for granted that the preferences of college men for rhythms will be in exact proportion to the size of the groups in such a list of favorite nursery rhymes as that considered in the previous section. Aside from other influences, it is quite possible that the impression left by the rhymes in the short meter pattern may be more lasting than that left by the larger groups of other sorts. A motion rhythm like that of "Peas porridge hot," which all children like to play, would probably have considerable advantage in this respect. It is more probable, however, that the fact noted indicates that some rhythms are better suited for certain purposes than for others, and that the choice of the tripodic form so prominent in the yells rests upon other and perhaps physiological reasons. Three explosive sounds can be uttered with more ease and satisfaction—for respiratory reasons probably—than a greater number (it is a common saying of those who invent yells that the test of excellence is to try them), and with more telling effect than a less number. In cheering, also, the abruptness of the sound is an important factor. A yell without marked pauses cannot possibly be so effective as one in which the pulses of sound are more sharply marked off from one another. An example will perhaps make this clear. The following yell, even when given by practiced voices, merges into a mere bellow of sound: Gloriana, Frangipanna, Indiana! Kazoo, Kazah! Kazoo, Kazah! Hoop Lah! Hoop Lah! State University, Rah! Rah! Rah! One of the more effective type in use in the University of California is—Rah, Rah, Rah, Califor-ni-a, U. C. Berk'lee, Zip, Boom, Ah! In so far as the first lacks in ex-

[1] Charles Ledyard Norton: Political Americanisms, p. 120.

plosive abruptness of rhythm, it loses in stirring effect. The abruptness will of course depend largely on the syllables that compose the yell, but, as appears from the tables given in the preceding section, the short meter pattern has an advantage over the others in having unmistakable pauses at the ends of the first two of its 3-stress lines.

It would be interesting if it were possible to trace the history of cheering. Antedating all college yells, so-called, we have the conventional form of it, which is known as "giving three cheers" or giving "three times three" for a person. This custom was observed at least as far back as the last century, and probably much earlier.[1] It is undoubtedly from this that the original nine rah yells of Yale and Harvard are derived. The question of, why *three* cheers is left unsettled by us with only a conjecture that long ages of practical experience developed this rhythmic form of sounds as the most satisfying and effective.

IV. Some Common Rhythms and the Words that have been Fitted to them.

The tendency of imaginative hearers to give a meaning and a verbal clothing to rhythmic sounds of any sort is everywhere in evidence, especially where there is the perception of conformity of the rhythm to one of the familiar metric patterns. Every one can recall instances. The telephone call of a veterinary surgeon is translated by another person on the same circuit into the perfect iambic line "my horse is sick; come quick, come quick." The rising bell in a large woman's college says to one "crawl out, crawl out, crawl out," and for meals "come in, come in, come in." To another it says "get up, get up, get up," and "quick quick, quick quick, quick quick," respectively. One person was told as a child that the schoolbell said "Helen, come to school," and has heard those words ever since when it rings. The dinner bell is reported by a masculine observer to say, "you hungry cusses, come in, come in; you hungry cusses, come in, come in."

Perhaps the most familiar instance is the interpretation of the ringing of church bells—a very different interpretation, however, in the case of the faithful churchgoer and in that given by the profane sluggard whose rest is thereby disturbed. This general trait has been elaborately worked out in a poem formerly much affected by elocutionists, beginning:

> "How sweet the chime of Sabbath bells,
> Each one its creed in music tells."

[1] Scots Magazine, in 1789, p. 356.

Each creed in successive stanzas receives its characterization in the language of the bell. Perhaps two instances will suffice:

> "Farewell, farewell, base world farewell,
> Say to the world farewell, farewell,
> Pealed forth the Presbyterian bell."

> "In after life there is no hell,
> No hell, no hell, no hell, no hell,
> Rang out the Universalist bell."

The railroad and steamer furnish an almost inexhaustible supply of meters and rhythms. Engineers of imaginative temperament listening to the voices of their engines endow them with the attributes of a distinct personality, and the traveller by steamer may hear in the throb of the screw: "Got yer trunk, got yer trunk, got yer trunk." Musicians find in such sounds suggestions for motifs as Leonardo found those for pictures in the mottled surfaces of tables. Berlioz is said to have derived the motive for the "Ride to Hell" in the "Damnation of Faust" from the clattering of an express train during a sleepless night. What shall be the meaning given to these rhythms depends on the emotional tone of the hearer. To one young romancer it is the machine spirit repeating with variations the words "Oh hurry up, oh hurry up, oh hurry up." To one who has been bereaved by its agency it is the threatening voice of a cruel monster. Kipling's ".007" covers "his one hundred and fifty-six miles in two hundred and twenty-one minutes"—"a hundred feet to each word."

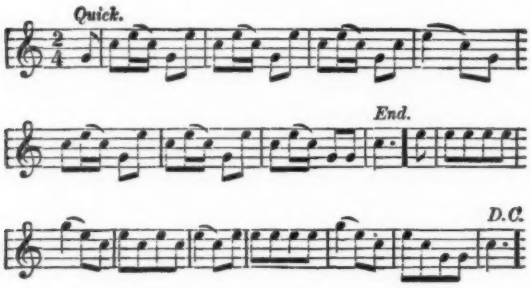
> "With a michnai—ghignai—shtingal! Yah! Yah! Yah!
> Ein—zwei—drei—mutter! Yah! Yah! Yah!
> She climb upon der shteeple
> Und she frighten all the people
> Singin' michnai—ghignai—shtingal! Yah, Yah."

To one listener the ticking of an alarm clock calls up the Lady of the Lake. The patter of rain, the crunching of frozen snow, and even the rhythm of ordinary walking bring a verbal accompaniment to the minds of some. This tendency has long been exploited by the humorist in the croaking of bullfrogs and the hooting of owls. Bird lovers interpret the notes of their favorites. The robin says "Cheer up, cheer up," the white-throated sparrow "See me! Peabody, peabody, peabody." The crowing of the cock to one means "Who the devil cares." To spur his flagging energy a farmer's boy was told that the meadow lark sings "Laziness will kill you," and has continued to hear these words in its song. To others they mean "My poor children."

The various metric patterns presented in the military bugle calls have furnished the best illustrations of the trait under

discussion. During the Civil and Spanish wars the following words were fitted to them:[1]

14. REVEILLE.

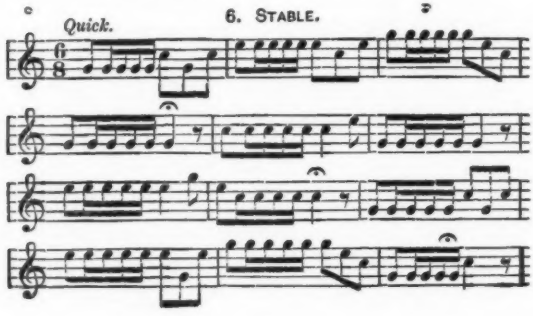


I can't get 'em up,
I can't get 'em up,
I can't get 'em up in the morning.
I can't get 'em up,
I can't get 'em up,
I can't get 'em up at all.

The captain 's worse than the sergeant,
The sergeant 's worse than the corporal,
The corporal 's worse than the private,
But the major 's the worst of all.

[Then repeat first part.]

6. STABLE.

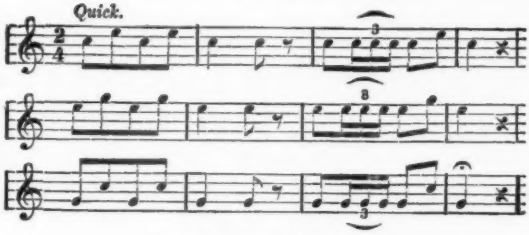


Come, come to the stable
All ye who are able,
Water your horses and get them some hay,

[1] For the electrotypes of the musical scores we are indebted to the courtesy of D. Appleton & Co., New York.

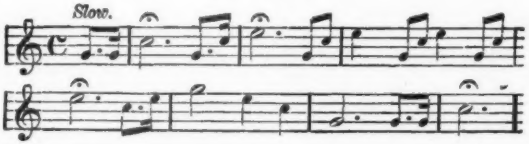
For if you don't do it
The colonel will know it,
And ho! for the guard house
The very next day.

18. MESS.



Soupy, soupy, soup, soup,
Without a single bean;
Porky, porky, pork, pork,
Without a streak of lean;
Coffee, coffee, coffee,
The vilest ever seen.

17. TAPS.



[The signal for lights out.]
Put it out, put it out, put it out, etc.

The words soldiers have put to this call when used in the burial services are:

"Love, good night, must thou go,
When the day and the night need thee so?
All is well.
Hasten all to their rest."

In the ordinary setting of words to music great liberty is taken with the quantity of the syllables used. In the present instances where words have been *fitted* to the music a more exact congruity may be expected, and the question may naturally be asked whether the notes represent with fair accuracy the true quantities of the syllables in question. This seems rather doubtful, especially in the case of the unstressed syllables. The cases are interesting, however, as showing the same rhythmic patterns fashioned from two sorts of rhythmic material.

LITERATURE.

La Folie: ses causes, sa thérapeutique, au point de vue psychique. Par TH. DAREL. Avec une préface du DR. E. GYEL. Geneva, M. Reymond; Paris, F. Alcan, 1901. pp. 196. Price, Fr. 3.50.

This essay, "un livre tout d'intuition," as the Editor terms it, is the outcome of a philosophy of 'psychism.' "The human individual is constituted by an extremely complex grouping of monads, themselves of very different stages of evolution. . . . . Such a group is naturally in a state of unstable equilibrium; and madness, that is to say the annihilation of the direction of the central monad, is the result of a rupture of equilibrium between the soul and the mental elements and between the mental elements and the 'astral' and material principle. The causes that predispose to insanity can all be traced to a lack of affinity between the soul and the secondary principles." We then have (1) hysteria, or insufficiency of central direction; (2) delirium of persecution, or rebellion of the mental elements from the ego, with subordination to the preponderant elements of the group; (3) mania and melancholia, or anarchy of the elements; and (4) dementia, or psychical disaggregation. The thesis is vigorously worked out; but most psychologists will prefer a less 'intuitive' account of the abnormal mind.

Socrate (*Les grands philosophes*). Par C. PIAT. Paris, F. Alcan, 1900. pp. 270. Price, Fr. 5.

This volume is the first of a series of works upon the leaders of philosophical thought, which will appear in quick succession under the general editorship of M. Piat. Kant, Avicenna, Malebranche, St. Anselm, St. Augustine, Descartes, St. Thomas Aquinas, St. Bonaventura, Maine de Biran, Pascal, Spinoza and Duns Scotus will form the subjects of the next following issues.

In the work before us, M. Piat treats of the life and teaching of Socrates in ten chapters, entitled respectively Social Surroundings, Youth, Vocation, Dominant Idea, Method, Ethics, Theology, Eschatology, Trial, and Influence. He writes pleasantly, and in popular vein; and, if he brings us nothing new, manages at least to cover his ground pretty thoroughly.

Experimental Psychology, by EDWARD BRADFORD TITCHENER. A manual of laboratory practice. Vol. I. Qualitative Experiments. Part I. Student's Manual. pp. 214. Price, $1.60. Part II. Instructor's Manual. pp. 456. Price, $2.50. The Macmillan Co., New York, 1901.

It is a pleasure to announce the appearance of this very important and long expected work, due notice of which will appear later. It was a misfortune, however, that the two parts did not appear together.

A Memorial of George Brown Goode. Annual Report of the Smithsonian Institution. U. S. National Museum, 1897, Vol. II. Washington, 1901, pp. 515.

The first thirty-eight pages of this interesting volume are devoted to abstracts of the remarks made by Gardiner Hubbard, Professors Langley, W. L. Wilson, H. F. Osborn, and W. H. Dall. Then follows

a twenty page memoir by Professor Langley, and the rest of the volume is devoted to papers on museums and the history of science in America, with a final bibliography of Goode's works. This book is made more interesting by full page plates of 109 of the most eminent men in the history of American science.

Götterglaube und Göttersagen der Germanen, von WOLFGANG GOLTHER. L. Ehlermann, Dresden, 1894. pp. 66.

This unique little book attempts a condensed statement of what the ancient Germans believed, and how they worshipped, and to present it all in historical perspective. The more important chapters are on belief in spirits, souls, elfs and giants, gods and the Supreme God of heaven, Wodan, Baldr, and other special deities, the creation and destruction of the world, divine service. The author evidently holds that this is not a bad faith to live and die by.

The Science of Life. An Outline of the History of Biology and Its Recent Advances, by J. ARTHUR THOMSON. Blackie and Son., Ltd., London, 1899. pp. 246.

This is an admirable compend of evolution in the field of biology, by a facile pen, with several dozen helpful titles in the way of bibliography.

Geschichte de Neugriechischen Volksschulwesens, von PAUL KIPPER. H. Starke, Grossenhaim und Leipzig, 1897. pp. 96.

This history of the Greek school system begins in 1827, when the present scheme was organized under French influence. It describes the gradual predominance of German influences and the development of the courses of lower and intermediate education, nearly up to its date, with copious references.

Erziehung und Erzieher, von RUDOLF LEHMANN. Weidmannsche, Berlin, 1901. pp. 344.

The chief topics discussed are the relations between education and heredity, habit, educational ideals, home, the departments, philosophy in the school. The latter chapter appears to the writer of this note the most important.

A Short Account of the Hebrew Tenses, by R. H. KENNETT. University Press, Cambridge. The Macmillan Co., New York, 1901. pp. 104. Price, $1.00.

The writer has found that students of Hebrew find special difficulty with tense, hence this primer designed to lead up to a fuller treatment of the subject in Driver's well known work.

The Books of the New Testament, by LEIGHTON PULLAN. Rivingtons, London, 1901. The Macmillan Co., New York, 1901. pp. 300. Price, $1.25.

This introduction is neither a mere handbook nor an elaborate treatise for specialists. It is conservative, yet has made ample use of recent critical investigation. It devotes a chapter to each of the main books with several interesting appendices.

A Text Book of Psychology for Secondary Schools, by DANIEL PUTNAM. American Book Co., New York, 1901. pp. 300.

This is an interesting book by a revered teacher of long experience. It is lucidity itself, as well befits the normal classes to whom the author ministered. His mind was conservative, and while most of his work is devoted to what some now call the old psychology, there are plenty of illustrations from the new, especially on some of the senses,

dreaming, hypnotism, and with suggestions for apparatus and experiment.

La Philosophie de la Nature chez les Anciens, par CH. HUIT. Thorin et Fils, Paris, 1901. pp. 583.

The author considers the relations of nature to religious thought among Hebrews, Persians, Egyptians, Chinese, and Hindus; the relations of nature to poetic sentiment in the literature of ancient Greece and Rome; and then treats of scientific and metaphysical nature study among ancient philosophers.

The Riddle of the Universe at the Close of the Nineteenth Century, by ERNST HAECKEL. Translated by Joseph McCabe. Harper and Brothers, New York, 1901, pp. 391.

There is a swan song which marks "the close of my studies on the monistic conception of the universe." The author renounces a system of monistic philosophy he had planned on account of growing age and weakening strength, and adds that "I am wholly a child of the nineteenth century and with its close I draw the line under my life's work." He here treats of life development, the soul, immortality, substance, nature, belief, monistic religion and ethics, in an easy and very interesting way interspersed by many fascinating reminiscences.

Problems of Evolution, by F. W. HEADLEY. Duckworth & Co., London, 1900. pp. 373.

First the writer tries to show that Lamarck is wrong as to the moulding influence of the environment, but he pleads for a world wide tendency to vary, and with natural selection as a regulating principle, so that all species, even the lowest, in a sense pilot themselves and heredity is progressively limiting the range of variation. With man the same principles as with the lower creatures are still operative, but others come in, so that civilization, though quite distinct from, is still guided by evolution. A discussion of the conditions that favor and oppose progress leads to a final chapter on China as unprogressive. The chapters on the interaction of species; the influence of the individual on the evolution of the race; on isolation; on moral, religious and intellectual evolution, are interesting and suggestive.

Evolution of To-day, by H. W. CONN. G. P. Putnam's Sons, New York, 1899. pp. 342. Price, $1.75.

These pages are intended for those who having an interest in the question have neither the time nor the requisite knowledge of biology to read the numerous special discussions on the various phases of the subject. Hence the chapters are—what is evolution; are species mutable; classification of the organic world; life during geological ages; embryology; geological distribution; Darwin's explanation of evolution; more recent attempts to explain it; the evolution of man.

Studies Scientific and Social, by ALFRED RUSSEL WALLACE. 2 vols., pp. 532, 535. Macmillan & Co., London, 1900.

These two volumes are mainly reprints of more important articles, which the author has contributed to reviews and other periodicals during the thirty-five years ending in 1899. He has, however, introduced many copious illustrations which modify and frequently enlarge the original articles. The range of the author's studies is perhaps better seen here than in any of his works. He has grouped the 42 essays under the larger headings of earth studies, descriptive zoölogy, plant and animal distribution, theory of evolution, anthropology, education, politics, the land problem, ethics and sociology.

Animal Behavior, by C. LLOYD MORGAN. Edward Arnold, London, 1900. pp. 344. Price, $3.00.

In attempting to revise the author's "Animal Life and Intelligence" for a new edition, it appeared "that the amended treatment would not fall conveniently under the previous scheme of arrangement," and he has, therefore, given us a new book. A few passages from the older work and some from his "Habit and Instinct" have been introduced or summarized. He treats organic behavior, consciousness, instinctive behavior, intelligent behavior, social behavior, feelings and emotions, and evolution of animal behavior. The work has a few illustrations.

The Laws of Orientation among Animals, by G. REYNAUD. Annual Report of the Smithsonian Institution, 1898. Washington, 1899. pp. 481–498.

In this discussion the author succeeds to his own satisfaction in eliminating all the theories that imply that homing birds or dogs are influenced by knowledge of the country, by geometrical triangulation, by the heavenly bodies, magnetic current, etc.; but thinks that in wintering creatures follow the lines of greatest attraction and least resistance, and the same principle brings them back.

Supériorité des Animaux sur l'Homme, par le DOCTEUR PH. MARÉCHAL. Fischbacher, Paris, 1900. pp. 228.

Animals are superior to us sometimes in senses, locomotion, metamorphosis and sex; in fighting the inconvenience of viviparousness; in having a more condensed mode of communication and often a more finished social organism. The author finds in animals the rudiments of medicine, religion, morals, science, and metaphysics. The writer's style is interesting and he marshals a large body of facts current in the literature of the subject in support of his hypothesis.

Dogs and Savages, by B. LANGKAVEL. Annual Report of the Smithsonian Institution, 1898. Washington, 1899. pp. 651–675.

This is a valuable digest of a very voluminous literature on the relations in many savage lands between dogs and human beings.

Gemüt and Gemütsbildung, von PAUL RÖNTGEN. Jos. Kösel'schen, Kempten, 1900. pp. 368.

These social and pedagogical studies are prefaced by an interesting statement of what the author conceives the *gemüt* is; its relation to knowledge, will, love, the heart, religion, character, while only the last 120 pages are pedagogical. It abounds in judicious quotations.

The Science of the Emotions, by BHAGAVÁN DÁS. Theosophical Publishing Society, London, 1900. pp. 183.

This work is inscribed to Annie Besant, by whose wish and under whose guidance it was written. It treats the factors of emotion; its essential nature; different kinds and subdivisions; relations of emotions; virtue and vice; complex emotions and their correspondents; emotions in art; in human life; and the high applications of the science of emotions. It is based largely upon Sanskrit sources, and comes into little contact at any point with occidental psychology.

La Foule Criminelle, par SCIPIO SIGHELE. F. Alcan, Paris, 1901. pp. 300.

This is a totally recast and enlarged second edition of the author's famous work, with new illustrative cases appended, and considerable reconstruction of the chapters.

An Essay on Personality as a Philosophical Principle, by WILFRID RICHMOND. Edward Arnold, London, 1900 pp. 219.

This essay is intended to illustrate a philosophical principle and not to establish a philosophical conclusion. Fellowship, the author thinks, would be quite as good a title. He first discusses experience and personality; their meaning and definition; then the faculties of personality, feeling, will and intellect. Perhaps the best chapter is the last on emotions, of which he makes love, and especially religious love, the highest type.

The Human Nature Club. An Introduction to the Study of Mental Life, by EDWARD THORNDIKE. Longmans, Green and Co., New York, 1901. pp. 235. Price, $1.25.

This somewhat enlarged edition rather needs the author's warning that too much must not be expected of a book which tries to handle psychological questions without technical words and without presupposing a knowledge of elementary science. It does indeed tell little truth, but it touches upon most of the large themes in current psychology, but so lightly and with such incessant paraphrase of James that we can but question the author's pedagogic success.

The Philosophy of Friedrich Nietzsche, by GRACE NEAL DOLSON. The Macmillan Co., New York, 1901. pp. 110.

This is a critical exposition of Nietzsche's writings so far as they are concerned with philosophy and an attempt to point out their historical position. Riehl thinks no serious German writer so widely read, and while Miss Dolson refuses to accede him the foremost place in the thinkers of all time, which his disciples claim, he is not a charlatan taking himself seriously, but a significant figure among the philosophers of his quarter century. The entire literary movement, known as young Germany, acknowledges his leadership. Indeed he is not an isolated phenomena, but a part of the general intellectual movement of the last decades, and thus expressing clearly what many have dimly thought, only perhaps more radically. After a brief biography and outline, this writer treats of his æsthetic, intellectual and ethical periods successively, and finally of his relations to Schopenhauer, Hegel, the materialists and Neo-Kantians, and also his literary affinities.

Sexual Debility in Man, by F. R. STURGIS. E. B. Treat and Co., New York, 1900. pp. 432.

The chief features of this book are the author's advocacy of castration of certain lunatics under special conditions, and his vigorous opposition to the old belief that masturbation is the prelude to both mental and physical degeneration. The first three chapters are devoted to the anatomy and physiology of his subject, while the rest treats of morbilities.

Uchronie (l'Utopie dans l'histoire), par CHARLES RENOUVIER. F. Alcan, Paris, 1901. pp. 412.

This is a very curious and interesting apocryphal historical sketch of the development of European civilization, not as it has taken place, but as it ought to have taken place. This is set forth in the story of a certain occidental Middle Age, which commenced in the first and ended in the fourth Christian century, and then in a modern occidental history ending in our own century. In the sequel he shows with great artistic talent what would have occurred if certain eminent historical percentages had formed other resolutions than they did, and what incalculable calamities would have followed if things had been

at their worst. Thus, while the optimist may rejoice that history has on the whole been as fortunate as it has been, the pessimist, if his ideals coincide with those of the author, will find justification because things might have been so much better.

Essai critique sur le droit d'affirmer, par ALBERT LECLÈRE. F. Alcan, Paris, 1901. pp. 263.

The author here treats of the principles and methods of a normal theory of being and of knowledge and more specifically of the Eliatic standpoint, unreality, the relations between conscience and reality, phenomena and reality. The second part is devoted entirely to the science of the non real, including phenomena in its relation to time, space and number; science and the activity of the soul; and the conditions under which a normal science of non-being is possible.

Un Siècle Mouvement du Monde de 1800 à 1900. Librairie H. Oudin, Paris. pp. 914.

This work is published by a committee centering in the Catholic University of Parigi and approved by Cardinal Rampolla. It consists of three parts, under each of which ten or twelve topics are treated, each by a different writer. The parts are political and economic, the intellectual and the religious movement of the closing century. Among the most interesting are those on education, philosophy, mathematics, biology, geology, archæology, history, literature, fine arts, music, physics and chemistry, and the press.

Audition colorée et Phénomènes connexes observés chez des écoliers, par AUG. LEMAITRE. F. Alcan, Paris, 1901. pp. 169.

After an interesting chapter on photisms and personifications of numbers, letters, etc., the author gives a number of diagrams of number forms, etc., from various sources, and then studies in detail the synopsia of three interesting subjects with copious illustrations. His interest centers in an attempt to explain genetically these curious phenomena.

On Artificial and Temporary Colour-Blindness, with an examination of the colour sensations of 109 persons, by GEORGE J. BURCH. Phil. Trans., London, 1899, Series B. Vol. 191. pp. 1–34.

The results of the author are unfavorable to the theory of Hering, and confirm that of Young and Helmholtz, but indicate the presence of a fourth color sensation, namely blue, which Young was prepared to admit if experimental evidence could be found.

Greek Thinkers. A History of Ancient Philosophy, by THEODOR GOMPERZ. Vol. I, translated by Laurie Magnus. Charles Scribner's Sons, New York, 1901. pp. 610.

This work summarizes the labor of a lifetime and will be complete in three volumes. It is not written from the standpoint of any exclusive school. It is here at last realized that historical relief is significant and that an outline of the story of religion, literature, and the special sciences is indispensable to an understanding of the speculative movement. The ideal is the universal history of the mind of antiquity. All critical discussion is wisely referred to notes, and the present volume prepares the way for the second which will begin with Socrates, while the third will end with mystics, sceptics, and syncretists.

Lehrbuch der Physiologie des Menschen, von G. von BUNGE. Erster Band. Sinne, Nerven, Muskeln, Fortpflanzung in achtundzwanzig Vorträgen. F. C. W. Vogel, Leipzig, 1901. pp. 381.

This first volume treats of the physiology of the senses, nerves,

muscles, and sex in the form of 28 lectures, with 67 cuts. The author's excuse in yielding to the demand of his students to publish is that now-a-days there are so few physiologists who teach over the entire field. The work certainly is not lacking in pedagogical lucidity, in condensation, or in apparent familiarity with first sources.

Trattato di Psichiatria del BIANCHI LEONARDO. Napoli. Puntata I. L. 4. pp. 170.

This treatise, designed for the use of medical students, the first part of which is here published, promises to be one of the best compends in any language. We can but wish that the author had given more attention to his literary references in many cases, as he cites many names to some of which the reader would naturally be prompted to turn.

The Christian Doctrine of Justification and Reconciliation, by ALBRECHT RITSCHL. English Translation, edited by H. R. Mackintosh and A. B. Macaulay. Imported by Charles Scribner's Sons, New York, 1900. pp. 673. Price, $4.00.

No work since Schleiermacher's *Christliche Glaube*, in 1821, has caused so deep a movement in the field of theology as this monumental treatise, the first edition of which appeared in Germany in 1870–74. This is the English translation of the third volume of the third edition of the original, which presents on the whole the main features of the author's view. The first shows that Ritschl's theology had no place in the ordinary classifications of theological parties, and the second exhibits the biblical material of his doctrine here summed up under the leading captions of the conceptions of justification, its presuppositions, proof and consequences.

Clue. A Guide through Greek to Hebrew Scripture, by EDWIN A. ABBOTT. A. and C. Black, London, 1900. pp. 158.

This work attempts to indicate means for constructing a clue by which scholars may systematically find their way through any Greek translation from Hebrew back to the Hebrew original; secondly, to demonstrate that parts of the synoptic gospels are based upon a common Hebrew document; and thirdly, to give specimens of the manner in which the clue may be used so as to return from the gospels to the original Hebrew. The chapters of the first part treat the errors of conflation in the pentitude, and of the second those in the synoptic gospels.

The Christology of Jesus, by JAMES STALKER. A. C. Armstrong & Son, New York, 1899. pp. 298. Price, $1.50.

The writer treats his matter in six chapters—the importance of the teaching of Jesus, the Son of man, the Son of God, the Messiah, the Redeemer, the Judge; with two appendices, one on Wendt's untranslated volume on the teaching of Jesus and the other on the book of Enoch.

God's Education of Man, by WILLIAM DEWITT HYDE. Houghton, Mifflin & Co., Boston, 1900. pp. 252. Price, $1.00.

The author here attempts to indicate in a general way and also uses a single small section to point out in considerable detail the radical and far reaching change which is taking place in theological conceptions. The more theological introduction treats of the reorganization of the faith; chapter 1 of control by law; chapter 2 conversion by grace; chapter 3 character through service, while in the conclusion a somewhat alien matter on two types of ideals is added. These are Plato and Aristotle; Kant and Hegel; Arnold and Browning; Garrison

and Lincoln; Burne-Jones and Watts; and of missionaries Nott and Anderson, Hamlin and Livingstone.

Goethe's Selbstzeugnisse über seine Stellung zur Religion und zu religiös-kirchlichen Fragen, von TH. VOGEL. B. G. Teubner, Leipzig, 1900. pp. 242.

The self evidences of religion, Goethe finds, as interpreted by this book, in the impulsion toward the sublime found in reverence and worship; in the idea of God and nature; the worth of humanity; body and mind; working and warring; patience, renunciation, unrest, penance, immortality. His expressions also are summarized upon the following topics:—revelation and scripture, miracle, Christ, primitive Christianity, the visible and invisible, church, and church history.

Ethics: Descriptive and Explanatory, by S. E. MEZES. The Macmillan Co., New York, 1901. pp. 435. Price, $2.60.

This work is dedicated to the author's first teacher in philosophy, Professor G. H. Howison, and attempts to give a critical and methodical account of what morality and immorality really are. The chapter heads best indicate its scope. They are definition and methods; moral and non moral phenomena; subjective morality; voluntary action; the adult conscience; its psychic law; birth and growth of conscience in the child; in the race. The second part treats of objective morality, and more specifically the constituents and criteria of objective morality, courage, temperance, benevolence, justice, wisdom, welfare, the nature and value of morality. We have glanced critically at only the chapters on the growth of consciousness in the individual and in the race, because these are the newer topics and best calculated to reveal the author's method and his thoroughness, but both sections are aridly speculative and show almost no acquaintance with the interesting new literature in this field. In general the work moves in the sphere of the common speculative ethics of the schools.

The Evolution of Immortality, by S. D. MCCONNELL. The Macmillan Co., New York, 1901. pp. 204. Price, $1.25.

This is one of those exasperating books without an index or even a heading to its chapters, so that there is absolutely no point of approach. One must either read it through, or sample it, or give it up. We have done the two latter. There are to-day restless minds with an intellectual psychosis akin to that of tramps, who rove over the whole world of knowledge in a light easy going way; know a little of Darwin and have heard of Wundt and Haeckel; have dabbled in patrology; spice their pages well with poetic extracts, stray sentences from travellers, theologians, physicists, Bible critics, and historians; give long lists of great names, who think thus and so, but intersperse them in an uncritical way with popular modern writers utterly without authority; whose chapters might be placed in any other order, because there is no real continuity or progress. We by no means state that this author is thus described, but we do assert emphatically that from our standpoint his contribution to this most important subject is disappointing in the extreme, and that he shows throughout little or no knowledge of what seems to us an important contingent, viz., the anthropological and psychological treatment of his theme.

History, Prophecy and the Monuments, by JAMES FREDERICK MCCURDY. Vol. I, pp. 425; Vol II, pp. 433; Vol. III, pp. 470. The Macmillan Co., New York, 1896, 1897, and 1901. Price, $3 per volume.

The writer here attempts to cover all the period in Jewish history, which are illustrated by contemporary monuments, and seeks to get

the non prophetic and indirectly the prophetic history of Israel, with its historical occasions or antecedents. The scope of the work also involves an interpretation of the nature of prophecy, which can be only learned by a study of history. The ground covered in these three volumes is vast—the Northern Semites, Babylonians, Canaanites, Egyptians, Hittites, Aramaeans, Assyrians, the inner development of Israel, Chaldeans, Persians, etc. The author has striven to embody the main results of modern scholarship in this vast field, in which there is now such a rising tide of new interest, in a way entirely intelligible to the educated laymen, and, while in obvious sympathy with the modern and progressive views, there is nothing in the work that can offend the scholar of more conservative tendencies. These volumes constitute a valuable addition to the literature of the subject and will be sure to find a place in every well equipped library and study where this department is represented.

La France au Point de Vue Moral, par ALFRED FOUILLÉE. F. Alcan, Paris, 1900. pp. 416.

This vigorous author is profoundly dissatisfied with the moral condition of his native land, and feels that the present is a crisis for both morals and religion. The rapid progress of criminality, particularly among the youth, makes one of the strongest chapters in the book, while the discussion of the relations between education and democracy in France; the section on the education of adolescents and secondary education, lead to the conclusion that modern learning is not sufficient for moral reform. The writer evidently shares, as far as is consistent with his hearty patriotism, the fears of many of his countrymen, that France is now in danger of a moral decadence.

Reconstruction in Theology, by HENRY CHURCHILL KING. The Macmillan Co., New York, 1901. pp. 257. Price, $1.50.

"A new constructive period in theology, it may well be believed, is at hand. This book has been written with the earnest desire and hope that it may contribute something toward the forwarding of a movement already going on, a real spiritual reconstruction of theology in terms that should bring it home to our day." After discussing the spirit now needed in theology, the evidence of this growing need of reconstruction and its reasons, the author discusses the influence of the spirit of the New World on theology; its relations to science, evolution, historical and literary criticism; the position of Jesus; the personal relationship of religion and theology. If this volume does not materially contribute to the scientific development of the problems it teaches, it is an able statement of tendencies, and is also valuable as the utterance of a representative religious teacher.

Chapters from Aristotle's Ethics, by J. H. MUIRHEAD. John Murray, London, 1900. pp. 319.

Professor Muirhead has found the Nicomachean ethics a good introduction to many of the fundamental conceptions of moral philosophy. After outlining the science of ethics, and various opinions on the nature of happiness according to Aristotle, the author summarizes his teaching on its elements, the soul, virtue, courage, temperance, self-control, wisdom, friendship, and pleasure. The last one hundred pages are selected passages.

Constitution de l'Éthique, par E. DE ROBERTY. F. Alcan, Paris, 1900. pp. 223.

The scale of factors and their correlative values in the superorganic world, the relations of ethics to other sciences and to philosophy,—

these, with the author's introductory chapter explaining why he is not a positivist, constitute the book.

The Bee People, by MARGARET W. MORLEY. A. C. McClurg and Co., Chicago, 1900. pp. 177.

Miss Morley here writes with a little of the old charm that made The Songs of Life so justly popular, but which seemed to have faded from her Life and Love. She illustrates as well as writes, and this adds greatly to the effectiveness of her work.

The Ethics of Judaism, by M. LAZARUS. Translated from the German by Henrietta Szold. In four parts. The Jewish Publication Society of America, Philadelphia, 1900. Part 1, pp. 309.

This is a scholarly history, which carefully refrains from comparisons with other modes of thought save only in the case of von Hartmann, whose charge against every system of ethics based on theism is refuted. The three chapters of this volume are—the sources, principle, and character of Jewish ethics. The other three volumes will be awaited with interest.

Psychologie und Pädagogik des Kinderspiels, von G. A. COLOZZA. Oscar Bond, Altenburg, 1900. pp. 272.

The first chapter discusses play from the standpoint of psychology; the second gives its history in pedagogy; and the third details its pedagogic significance for suggestion, invention, physical training, development of eye, ear, touch, muscle, sense, memory, attention, feeling, etc. Fighting is commended in its season and with moderation. Solitude vs. sociability for children is discussed; toys, especially the doll, and their relation to the æsthetic feeling and imagination; the advantages and disadvantages of illusion and its relations to work are taken up.

The Philosophy of History, by A. SCHADE. A. Schade, Cleveland, O., 1899. pp. 437.

This is a most scholarly work, covering with its comprehensive formula the entire course of history, and thoroughly inductive in method. Progress in history is measured by the degree in which feeling, reason, and will are brought under the control of a free agent into equilibrium and proper mutual co-ordination. In this encyclopædia of all knowledge, Christianity and its ethics and church are made the center. The convenient marginal notes are a great aid to the reader.

Diseases of the Heart, Blood Vessels, Lymphatics, Blood and Ductless Glands. M. J. Breitenbach Co., New York.

This is an interesting and very condensed chart in the following columns:—name of disease, cause, symptoms, inspection, palpation, percussion, osculation, pulse, complication.

La Suggestibilité, par ALFRED BINET. Schleicher Frères, Paris, 1900. pp. 391.

The chapters are history, directive ideas, moral action, interrogation, imitation, conclusion. The work is a part of the author's larger plan of amassing material to shape an experimental psychology of the higher functions of the mind with a view to the differentiation of individualities. The two questions here treated are, whether effective suggestibility can be secured without hypnotism or to determine the degree of suggestibility, and the second to decide whether these or other tests of suggestibility are significant. Both these questions the author answers in the affirmative.

Die Mimik des Menschen auf Grund voluntarischer Psychologie, von HENRY HUGHES. J. Alt, Frankfurt, A. M., 1900. pp. 423.

In his introduction the author discusses the history and literature of mimic and gesture, and then takes up its psychological basis, under which he discusses his methods, its individual origin and differences, historical development from the animals up, and relation to art. The third chapter treats of movement of the face,—the forehead, eyes, nose, mouth, and ears; the fourth, movements of the body,—the head, arms, and legs; and the fifth treats the mode of expressing emotion under the rubrics,—the voluntary principle, composition of impulses, feelings, excitation of gemuth and will, mood, attention, inclination, and *achtung*.

The Mystic Self. Uncommon Sense vs. Common Sense, by RAVON. Chicago, 1900. pp. 70.

Mystic signs; a serpent; a radiant hand; three full-page photographs of Elfa among the people; in a magnetic sleep separating the two selves; and the physical self-dormant, the higher self away at work; a maze of quotations.

Dix Années de Philosophie. Études Critiques sur les Principaux Travaux Publiés de 1891 a 1900, par LUCIEN ARRÉAT. F. Alcan, Paris, 1901. pp. 179.

This is an attempt to outline the history of philosophy of the last decade under the rubrics—sociology, psychology, æsthetics, manners, religion, and doctrine. The writer names and briefly characterizes the leading works in this field, and has given us an interesting book.

Freedom and 'Free-Will,' GEORGE STUART FULLERTON. Popular Science Monthly, LVIII, 1900. 183-192.

"Freedom" implies absence of external compulsion; "Free-Will," absolute independence of objective and subjective influences. The former, all covet; the latter Mr. Fullerton denies. He, therefore, favors the view of determination, not fatalism, that we can do as we please, but that there is always a sufficient reason for the "as we please." The paper is a clear and popular restatement of Jonathan Edwards's view.

A. J. KINNAMAN.

Rhythm as an Aid to Voice-Training, by SARAH ALLAN JORDAN. Association Review, II, 16-19, Feb., 1900.

The author holds that man is born with the possibilities of a sense of rhythm, and that this can be made an educational factor in training the deaf mute to speak as the hearing child speaks, and in removing the defects of tone, modulations, and manner of speaking of the deaf child. The sense of rhythm is to be developed through bodily movements, and then applied in the speaking movements. The material for the training in speaking are, jingling rhymes, poetry, etc. As means for securing an understanding of differences in pitch, the author mentions the pipe organ and piano.

M. K. SMITH.

Essai sur l'esthétique de Lotze. Par A. MATAGRIN. Paris, F. Alcan, 1901. pp. 166. Price, Fr. 2.00.

Based upon Rehnisch's *Grundzüge*, 1884. Pt. i, discusses beauty,—its objective and subjective bases, its definition and modes. Pt. ii, discusses art in general and the arts in particular, giving Lotze's classification. A brief critical and historical study ends the volume.

Psychologie de l'invention. Par F. PAULHAN. Paris, F. Alcan, 1901. pp. 185. Price, Fr. 2.50.

An interesting and suggestive discussion (1) of invention, 'intel-

lectual creation,' in general, and its relation to affective and volitional processes, to imitation and routines; and (2) of the development of invention by evolution, transformation, and deviation, and of the corresponding developments of imitation and routine. General considerations of the place of invention in society: its relations to life, instinct, and chance, its range and philosophical significance.

Crime and Criminals. By J. S. CHRISTISON. Second ed., 1899. Chicago, J. S. Christison. pp. 177. Price, $1.25.

Expanded reprint of papers on 'Jail Types' published in the Chicago *Tribune*. Distinction between the insane (defective in reason); the moral paretic (defective in self-control); and the criminal (defective in conscience). Sketches of cases (including Windrath and Luetgert). Cause and cure of crime; prison treatment.

The Political Economy of Natural Law. By H. WOOD. Boston, Mass., Lee & Shepard, 1899. pp. 305. Price, 50 cents.

Conventional political economy is unpractical, and therefore of little service in actual experience. We must attack the labor problem in the light of natural law, and improvement must come through a better interpretation of (and conformity to) its immutable lines.

History of Ancient Philosophy. By W. WINDELBAND. Translated by H. E. Cushman New York, Chas. Scribner's Sons, 1899. pp. xv, 393. Price, $2.00.

We are glad to give a word of commendation, though tardily, to this excellent class-book of Greek and Hellenic-Roman philosophy. It is a valuable addition to the apparatus of philosophical teaching.

Sanity of Mind: A Study of its Conditions and of the Means to its Development and Preservation. By D. F. LINCOLN. New York, G. P. Putnam's Sons, 1900. pp. vi, 177.

Chapters on mental derangement, degeneracy, education, and self-education. Recommendation of "custodial care of the classes known as the insane, the feeble-minded or idiotic, the epileptic, inebriates, criminals, tramps, and paupers," with a view to restrict or wholly prevent the propagation of a new generation.

Hypnotism a Complete System of Method, Application and Use, Prepared by the Self-Instruction of the Medical Profession. By L. W. DE LAURENCE. Second ed., illustrated. Chicago, The Henneberry Co., 1901. pp. 256. Price, $1.50.

Magic, White and Black: the Science of the Finite and Infinite Life, Containing Practical Hints for Students of Occultism. By FRANZ HARTMANN. Sixth ed., revised. New York, The Metaphysical Publ. Co., 1901. pp. 292. Price, $2.50.

Neither of these books has any scientific value. The former lays down practical rules, from the platform standpoint, for the induction of the hypnotic state: the chapter on the psychology of hypnosis gives no hint that the author knows anything of the physiology or psychology of his subject. The latter invites its readers to "rise mentally into the highest regions of thought and remain there as its permanent residents," or, more concretely, to raise the magic wand of their wills and still the tempests raging in the astral plane. It is significant that both books are, apparently, finding an extended sale.

Fact and Fable in Psychology, by JOSEPH JASTROW. Houghton, Mifflin & Co., Boston and New York, 1900. pp. xvii+375. Price, $2.00.

To speak of a scientific work in certain circles as "popular" is to

damn it and that without even faint praise. At the same time if science is to do its work for mankind, its standpoints and general results must somehow be made accessible to all intelligent men. There is need and opportunity for the genuine interpreter of science as well as for the specialized producers of new facts. It would therefore be in hearty commendation if the reviewer should describe this collection of Prof. Jastrow's previously published essays as a popular work in the best sense. The matter and method are interesting, and both the scientific attitude and dignity of presentation are preserved throughout. But the book is considerably more than that. All of the essays represent a fresh and first-hand treatment to the questions considered, and several embody the substance of valuable original contributions. The titles of the main group show sufficiently both the topics and the range of the treatment: The Modern Occult. The Problem of Psychical Research. The Logic of Mental Telegraphy. The Psychology of Deception. The Psychology of Spiritualism. Hypnotism and its Antecedents. The Natural History of Analogy. The Mind's Eye. Mental Prepossessions and Inertia, and A Study of Involuntary Movements. To have brought together in one readily accessible place so much that bears upon such an important chapter of anthropological psychology is itself a contribution. Prof. Jastrow's attitude toward "Psychic Research" (to use one term for the whole group) is that of the majority of competent authorities, namely, that the phenomena are worthy of study; that they can often be brought into line with known principles of physics, physiology and psychology; and that the presumption is overwhelmiug, even in the most remarkable and apparently inexplicable cases, that these also would fall into line, could absolutely full and reliable data be obtained. The last essay in the book is on the Dreams of the Blind, and furnishes an excellent indication of what might be expected from a thorough study of the psychology of defectives by a competent hand. This and the essay on Mental Prepossessions and Inertia are full of pedagogical suggestions.

E. C. S.

Recent Advances in Psychology. By E. B. TITCHENER. International Monthly, August, 1900.

In fourteen pages the writer presents a few points in vindication of the "new psychology," reviews and comments upon some of the results achieved in the fields of sensation and perception, attention and feeling, notes some features of the progress in genetic, animal and social psychology, and makes critical reference to some of the recent literature.

F. H. SAUNDERS.

The Psychology of Crazes. By G. T. W. PATRICK. Pop. Sci. Mo., LVII, No. 3, 1900. pp. 285-294.

The hypnotic phenomena and the reversionary mentality and morality exhibited by individuals in mental epidemics and crazes are due to the fact that the unusual excitement accompanying excessive emotion exercises an inhibitory effect upon the higher brain centers. The physical phenomena so common in mental epidemics tend to confirm this theory, for in excessive emotion the unusual excitement in the lower brain centers finds its expression through the motor channels. The last part of the article is devoted to the application of the theory to special cases taken from history.

The Angle Velocity of Eye Movements, by DODGE AND CLINE. Psychological Review, March, 1901. pp. 145-157.

After brief critical illusion to the methods of Volkmann, Lamansky and Huey in this field the authors state in five points the ideal ex-

perimental requirements for apparatus to record eye movements. Their own apparatus consisted of a sensitive film which could move easily in the vertical plane behind a narrow slit in the plate-holder of a camera. In the experiments the movement of a bright vertical line reflected from the cornea was photographed on the moving film. The averages of the measurements are compared with those obtained by Huey, and the peculiarities of the times found by the latter are accounted for as due to the necessary inertia of his apparatus.

F. H. SAUNDERS.

Psychological Observations of Spiritism. By TH. FLOURNOY. Reprint from the Proceedings of the International Congress of Psychology, Paris, 1900.

The author deprecates the attitude of regular science towards the phenomena loosely grouped under the term "Psychical Research," and holds that psychology would do well to investigate this subject concerning the nature of which the number of earnest inquiries is constantly increasing.

Prof. Flournoy has himself made a series of investigations, insufficient as a basis for generalizations, but sufficient to justify a distrust in the doctrines of Spiritism and Occultism. In no case has he found a single instance in their favor. A vast majority of cases may be referred to *unconscious perception and latent memory* by means of which material is preserved which may be used later by the "Subliminal Imagination" in constructing fictions singularly independent of the minds in which they originate. The facts given refer to his already previous use of Helene Smith described in his work "From India to the Planet Mars." (See this *Journal*, XI, 428, and XII, 265.)

The author recommends a careful psychological and logical study of the fallacies by which mediums and adepts deceive themselves.

MARGARET K. SMITH.

THE 'MIND' ASSOCIATION.

Owing to the death of Professor Henry Sidgwick, who had borne the financial responsibility for the conduct of *Mind* since 1892, as Professor Alexander Bain had borne it from 1876-1891, there has been formed in England a 'Mind' Association, the object of which is to make the journal independent of private liberality, and to put it upon a sound financial basis. The membership fee is one guinea, in return for which each member of the Association receives a copy of *Mind*. Though the Association is primarily a body of subscribers, it has the secondary function of organizing and stimulating philosophical interest. The leading British philosophers of the day are now members of the Association, and it may be confidently anticipated that they will co-operate in making *Mind* thoroughly representative of every side of philosophical thought.

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CORRESPONDENCE.

MY DEAR DR. TRIPLETT:

In a letter recently received, Prof. Dessoir takes exception to the statement in your letter to the Editors in the October number of the *Journal* "that several citations have been credited to Dessoir that are to be found in Jastrow's original paper," and points out that he gives Prof. Jastrow credit by referring in foot notes to two of his papers, and further that, in two out of the three instances in which you refer to his own work, you cite passages to the formulation of which he was certainly led by his own personal observations. In the third instance you have apparently been misled by a translation which is defective at this point, or possibly by a misplaced reference sign which occurs at the same place in the original, if you consulted that. The matter is perhaps a small one, but in view of your statement above quoted, some further explanation would seem desirable.

Yours very truly,
E. B. TITCHENER.

MY DEAR PROF. TITCHENER:

I gladly avail myself of the opportunity of your letter to correct the careless statement in my letter to the *Journal*. It is true that for "several" I should have written "one;" I regret to have been thus inaccurate. In this one instance, however, the wording of Prof. Dessoir's and Prof. Jastrow's articles is practically identical, and the inference was natural that the earlier writer should have the credit. Reference to Prof. Dessoir's original paper shows the translation to be defective, as you say—though at the time of writing I was not aware of it—and also the misplacement of the reference, for which of course I am hardly responsible.

Yours very truly,
NORMAN TRIPLETT.

BOOKS RECEIVED.

CLAPARÈDE, ED. Les animaux sont—ils conscients? Eggimann & Cie, Genève, 1901. pp. 24.

DAREL, TH. La folie, ses causes, sa thérapeutique au point de vue psychique. Avec une préface du Dr. E. Gyel. F. Alcan, Paris, 1901. pp. 196. Price, Fcs. 4.

DE LAURENCE, L. W. Hypnotism. The Henneberry Co., Chicago, 1901. pp. 256. Price, $1.50.

DOLSON, GRACE NEAL. The philosophy of Friedrich Nietzsche (Cornell Studies in Philos., No. 3). The Macmillan Co., New York, 1901.

FLOURNOY, TH. Observations psychologiques sur le spiritisme. Extrait des Comptes-Rendus du IV e Congrès Int. de Psy. F. Alcan, Paris, 1900. pp. 11.

HARTMAN, FRANZ. Magic, white and black. The science of finite and infinite life containing practical hints for students of occultism. The Metaphysical Pub. Co., New York, 1890. 6th ed. pp. 292. Price, $2.50.

JOLY, HENRI. Malebranche (Les grands philosophes). F. Alcan, Paris, 1901. pp. 296. Price, 5 fcs.

MASON, R. OSGOOD. Hypnotism and suggestion in therapeutics, education, and reform. H. Holt & Co., New York, 1901. pp. 344. Price, $1.50.

Saunders's Medical Hand-Atlases. The Nervous System, by Dr. C. Jakob. Edited by Dr. E. D. Fisher. From the 2nd revised German edition. W. B. Saunders & Co., Philadelphia, 1901. pp. 218, with 83 plates. Price, $3.50.

THORNDIKE, EDWARD. The human nature club. An introduction to the study of mental life. Longmans, Green & Co., New York, 1901. pp. 235. Price, $1.25.

TITCHENER, EDWARD B. Experimental psychology. A manual of laboratory practice, Vol. I. Qualitative experiments, Part I, Student's Manual. pp. 214. Price, $1.60. Part II, Instructor's Manual. pp. 456. Price, $2.50. The Macmillan Co., New York, 1901.

WOOD, HENRY. The political economy of humanism. Lee & Shepard, Boston, 1901. pp. 319. Price (cloth), $1.25.

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All communications to the editor from Europe should be addressed to: G. F. STOUT, *The University, Oxford*; those from America to E. B. TITCHENER, *Cornell University, Ithaca, N. Y.*

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